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NOVEMBER, 1894.

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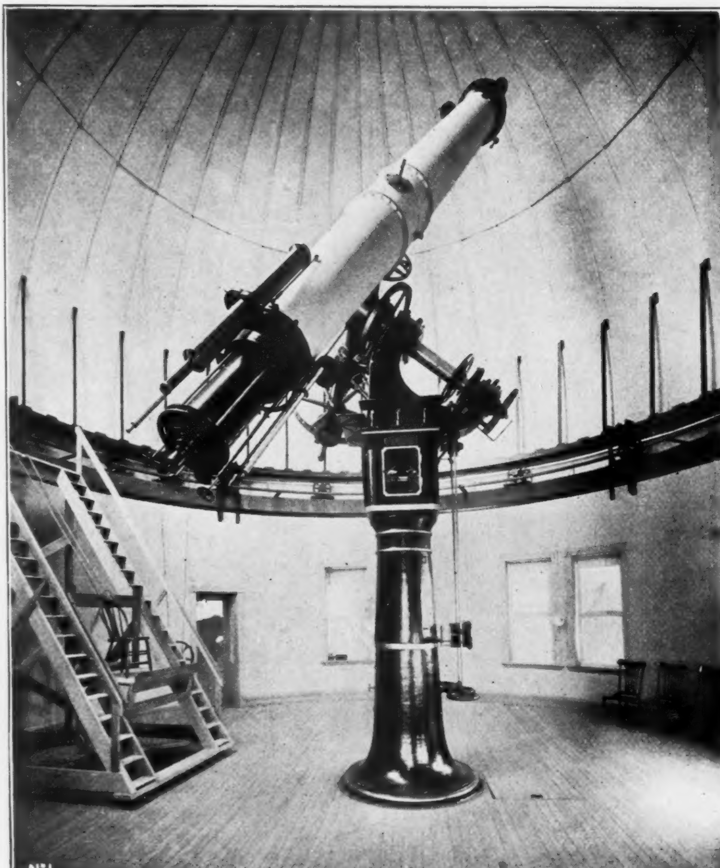
PLATE XXXI.



DENVER EQUATORIAL, 20 INCHES APERTURE.

MOUNTED BY G. N. SAEGMULLER, WASHINGTON, D. C.

ASTRONOMY AND ASTRO-PHYSICS, No. 129.

Astronomy and Astro-Physics.

VOL. XIII, No. 9. NOVEMBER, 1894. WHOLE No. 129.

General Astronomy.

THE 20-INCH EQUATORIAL OF THE CHAMBERLIN OBSERVATORY.*

H. A. HOWE, DIRECTOR.

The work of mounting the twenty-inch equatorial of the Chamberlin Observatory was begun in July, and the instrument is now in fair shape for use. The fact that the writer was able to get the instrument together without mistakes, and without the help of any skilled mechanics, speaks well for the care which was exercised in fitting and marking every piece in the shop. As this is the first large mounting of Mr. Saegmuller's construction, which has been set up in this country, astronomers will be interested to know about its peculiarities, together with the excellencies and the faults (if any) of its construction. First, however, for the object-glass:

THE OBJECTIVE.—The discs for this were obtained of Feil and were figured by Alvan G. Clark. They are well-nigh perfect specimens of optical glass: the crown lens is free from striæ, and the writer could find only three or four small ones in the flint lens. No polarization was shown by the ordinary test by reflected light, using a Nicol's prism. There is but one noteworthy bubble which is a millimeter and a half in diameter. The color-correction is better than the writer expected with so large a glass of the usual type, and the defining power is exquisite.

GENERAL DISCRIPTION OF THE MOUNTING.—The pier on which the instrument stands is built of a tough sandstone, being faced with dimension-stone, and filled with heavy rubble work. Its foundation is grout. The pier is 16 ft. square at the base, 12 ft. square at the top, and 25 ft. high, its base being 12 ft. below the surface. Into this pier are let three steel bolts, 9 ft. long and 3 inches in diameter. Their heads lock into horizontal foot-plates 2 ft. square, imbedded 7 ft. deep in the masonry. On top of the pier lie three similar plates, through which the bolts run, and to which they are held by very heavy nuts. On these bolts is sup-

* Communicated by the author.

ported the 7000 lb. casting (shown in cut), which formed the lowest section of the pillar, its top being nearly flush with the floor of the dome room. The adjustment of the instrument in latitude is made by lifting the entire column by means of the adjusting nuts on the north bolt. Upon this massive tripod stands a bell-shaped casting 5 ft. in diameter at the base and about 5 ft. high, to which is fastened by bolts running through internal flanges a second casting, on which in turn stands the square clock case. The adjustment in azimuth is beneath the floor, the bell-shaped casting being rotated by three pairs of opposing adjusting screws. The clock box can be shifted in azimuth (without adjusting screws)and is held in place by four set screws inside the pillar. The headstock is of a peculiar form, and projects far to the south of the pillar, so that the centre of gravity of everything above the clock-case is well in toward the geometrical axis of the pillar. The weight of the entire instrument is 25,000 lbs.

ANTI-FRICTION DEVICES.—The polar axis is a fine piece of Midvale steel resting in phosphor bronze bearings. The friction at the upper bearing is relieved by a set of six hardened steel rollers, each a foot in diameter and a quarter of an inch thick, which stand vertically side by side on the same axis. This axis is supported in an anti-friction bearing composed of small hardened steel cylinders. The system of rollers is nearly under the center of gravity of the moving portion of the instrument, and is pressed upward by a powerful bar spring inside of the headstock. Any desired tension is put upon the spring by means of a worm-gear, and the polar axis may thus be lifted entirely off its upper bearing.

The comparatively slight tendency of the lower end of the polar axis to rise is counteracted by a friction roller placed above it. The end thrust of this axis is small and is taken by a ball bearing at the lower end of it.

The declination axis runs in plain bearings, but the end thrust is taken by a ball-bearing at each extremity of the axis. The ball-bearing at the small end of the axis is adjustable and firmly secured by a set-screw. A practical advantage of having plain bearings on the declination axis is that when the instrument is near the meridian, so that there is very little pressure on the ball-bearings, the friction is sufficient to keep the instrument from rotating when the micrometer is put on or taken off. Thus no manipulation of the counterpoises is necessary. The large screw on which the counterpoises for declination are strung is not a continuation of the axis, but of the sleeve.

Driving Clock.—The driving clock has a Young's double conical pendulum, the friction shoes of which are shod with vegetable fibre. The vertical spindle which carries the pendulum, carries also near its lower end a horizontal wheel, on the lower face of which are set two diametrically opposite armatures, which revolve over opposite pairs of helices, for electric control. The pendulum makes two revolutions in a sidereal second, and the helices are supposed to quicken or to retard its motion, as may be necessary. The clock train carries a chronograph which may be used either for regulating it, or for ordinary noting of time. The clock may be started, stopped, or wound from the floor, and runs so admirably that an electric control seems almost a superfluity. An electric motor for winding the clock is in contemplation. In winter heated air from a room below rises inside the pillar, and keeps the clock warm by day and by night.

Main Circles.—Each vernier of the hour circle is read from the floor by a reading telescope near the dial box on the south face of the pillar; the smallest reading is one second, but half a second may easily be estimated. The verniers of the declination circle are read from the eye-end by two telescopes, the smallest reading being 5 seconds of arc. The divisions on both circles are exceedingly satisfactory in point of sharpness and distinctness.

Setting Circles.—The observer, when on the floor, sets the instrument to any desired right ascension and declination by turning the hand-wheels on the south side of the pillar, and reading the two dials contained in the large cylindrical box, which is above them, on a level with the eye. Each dial hand moves at double the angular speed of the corresponding axis. The declination dial is figured from 0° to 90° each way, the smallest space being 1°. The right ascension dial has five-minute spaces, and is driven by an eight-day clock. Notwithstanding the large number of gears involved in driving this mechanism, the total back lash is so small that a star of known coördinates is brought near the center of the finder at once.

It is important to have another system of setting circles visible from the eye end when the observer is on the north side of the pillar. This system consists of a 4 ft. circle on the declination sleeve which is read by the naked eye of the observer at the eye-piece to the nearest quarter of a degree with entire ease, and a 3 ft. circle on the north side of the clock box, which is similarly read to the nearest minute of hour-angle.

Illumination.—As the entire building is lighted by an alternating current at a pressure of 50 volts, this current has been uti-

lized for the two-candle power lamps, which illuminate the verniers of the main declination circle, the large setting circles, the micrometer, the hand lamp, etc. The main hour circle is lit up by two lamps of 16-candle power each, which are so placed that they light up the dials south of the pier as well. As the voltage of the house current is too high for the small lamps, it is run through a special converter made by Mr. E. G. Richardson, of Denver, and presented by him to the Observatory. The converter carries a switch by which the voltage of the secondary current is made to suit the small lamps, which are arranged in parallel, and are thus as easy to control as the house lamps. The light is steady, and there are no batteries to require attention. This method of illumination is so eminently satisfactory that it is urgently recommended to all Observatories which have access to an alternating current.

The converter is inside of the pillar, about 4 feet above the floor, and is reached through a large opening on the north side of the pillar. The secondary wires run up from the converter through the clock case, thence out of a hole in the nose of the headstock, up through the polar axis (which is hollow) into the declination sleeve, emerging at the inner end of the sleeve, and being there attached to a series of concentric rings. Springs fastened to the telescope tube, and pressing against these rings, lead the current to all required points on the tube. All switches are placed just where they ought to be, and the writer expects great satisfaction from the completeness and easy manipulation of the electric lighting.

THE EYE-END.—To the end of the main sheet steel portion of the tube is attached a short cylindrical casting upon which rotates a spectroscope jacket, similar to that on the Lick telescope. In order to adapt the tube to photography, the entire eye-end has been made to slide upwards a distance of about 3 feet, being guided by four steel rods, which run in eight guiding lugs within the casting which supports the spectroscope jacket. For visual work this sliding piece is pulled out as far as possible, and for photography it is thrust clear home, so that the photographic focus lies outside of it. In either position the sliding rods are held by clamp screws. At the lower end of the sliding piece is attached by a bayonet joint the tail-piece proper, consisting of the focussing tubes. The tail-piece has lateral adjusting screws, so that the sight line may be made perpendicular to the declination axis. There is but one finder, of five inches aperture.

MICROMETER.—This attachment varies in some particulars

from the ordinary American form. The verniers and the pinion for rotation in position angle are fixed, while the position circle revolves. Thus the observer can always find the pinion and the verniers, without loss of time. The circle which is 9 inches in diameter is divided to each tenth of a degree, and can be read by the verniers to hundredths if desired. Parallel to the movable micrometer wire is a system of wires, spaced at distances of 5 minutes of arc, for facilitating observations of comets or asteroids. There is but one fault to be found with the micrometer, namely that in certain positions the ends of the box are over the verniers, making them inconvenient to read. It is only just to the maker to state that he has promised to remedy this defect, together with any others which the observer may discover, after using it awhile.

Adaptation to Photography.—The crown lens of the objective is reversible, the two lenses being then separated by several inches. To accomplish this the telescope is pointed to the nadir, and the lower end of the tube is fastened to the pillar by a simple device. A reversing carriage is then run under the objective, so that the crown cell, which weighs about 150 lbs. is safely taken off, turned over and put back. When one wishes to photograph, the 5-inch finder and the entire system of handles for clamping and executing slow motions in right ascension and declination are slid up the side of the tube so as to be out of the way of the photographer though still usable. The tail-piece is then removed, and the plate-holder attached in its place by a bayonet joint. The plate-holder is movable in both right ascension and declination by five screws, the back lash being controlled by powerful springs.

For "following" there is a photographic finder, the objective of which is 5 inches in aperture, and is mounted on the outside of the main telescope, close by the 20-inch glass. The eye-end of the photographic finder is a small micrometer, which is attached to the plate-holder by a sliding mechanism, which allows the micrometer to be moved quite a distance in right ascension or declination or both, till a star suitable for "following" is found, and placed at the intersection of the spider webs. It is hoped that the displacement of the image on the photographic plate by changes of refraction, differential flexure of the tube, etc., will be practically the same as the displacement of the star which is used for "following." If there is any twisting of the plate about the line of collimation as an axis, it may be possible to detect it by turning the position-circle of the micrometer so that one of the

spider-webs shall bisect two stars at opposite sides of the field of view. No mechanism has been provided for correcting such a twist, but should the twist be discovered, it will be easy to attach the plate-holder to the rotating spectroscope-jacket, the motion of which is controlled by a worm.

RIGIDITY.—As it is well known that Mr. Saegmuller strives to build his instruments as light as is consistent with proper strength, some astronomers have feared that a large telescope of his construction might lack rigidity. This mounting is not open to such a charge, and must be considered as reflecting great credit upon its maker.

THE OBSERVING CHAIR.—This is 13 ft. high, 6½ ft. wide and 9 ft. deep. The platform (4 ft. by 3) slides up and down on four heavy trunk rollers, and is supported by a three-quarter inch Manila rope which takes a turn and a half around a six-inch oak drum; it is so counterbalanced as to require only the pressure of one finger to raise it. If two or three heavy persons are to be on the platform at once, an extra turn of the rope around the drum gives security against sliding downward. The chair is mounted on four of Martin's truck castors, which are equipped with anti-friction wheels, so that they rotate about their vertical spindles easily. A ring of iron concentric with the top of the wall of the room, and 3 ft. less in diameter keeps the chair from running against the electric lights, etc., on the wall. The chair works very satisfactorily.

UNIVERSITY PARK, Colo.

THE PLANET MARS.*

GIOVANNI SCHIAPARELLI.

As our chart demonstrates, in its general topography Mars does not present any analogy with the Earth. A third of its surface is occupied by the great *Mare Australe*, which is strewn with many islands, and the continents are cut up by gulfs and ramifications of various forms. To the general water system belongs an entire series of small internal seas, of which the Hadriacum and the Tyrrhenum communicate with it by wide mouths, whilst the Cimmerium, the Sirenum and the Solis Lacus are connected with it only by means of narrow canals. We shall notice in the first four a parallel arrangement, which certainly is

* Continued from page 640.

not accidental, as also not without reason is the corresponding position of the peninsulas of Ausonia, Hesperia, and Atlantis. The color of the seas of Mars is generally brown, mixed with grey, but not always of equal intensity in all places, nor is it the same in the same place at all times. From an absolute black it may descend to a light grey, or to an ash color. Such a diversity of colors may have its origin in various causes, and is not without analogy also upon the Earth, where it is noted that the seas of the warm zone are usually much darker than those nearer the pole. The water of the Baltic, for example, has a light, muddy color, that is not observed in the Mediterranean. And thus in the seas of Mars, we see the color become darker when the Sun approaches their zenith, and summer begins to rule in that region.

All of the remainder of the planet, as far as the north pole, is occupied by the mass of the continents, in which, save in a few areas of relatively small extent, an orange color predominates, which sometimes reaches a dark red tint, and in others descends to yellow and white. The variety in this coloring is in part of meteorological origin, in part it may depend on the diverse nature of the soil, but upon its real cause it is not as yet possible to frame any very well grounded hypothesis. Nevertheless, the cause of this predominance of the red and yellow tints upon the surface of ancient Pyrois is well known.* Some have thought to attribute this coloring to the atmosphere of Mars, through which the surface of the planet might be seen colored, as any terrestrial object becomes red, when seen through red glass. But many facts are opposed to this idea, among others, that the polar snows appear always of the purest white, although the rays of light derived from them traverse twice the atmosphere of Mars under great obliquity. We must then conclude that the arean continents appear red and yellow, because they are so in fact.

Besides these dark and light regions, which we have described as seas and continents, and of whose nature there is at present scarcely left any room for doubt, some others exist, truly of small extent, of an amphibious nature, which sometimes appear yellowish like the continents, and are sometimes clothed in brown, (even black in certain cases) and assume the appearance of seas, whilst in other cases their color is intermediate in tint, and leaves us in doubt to which class of regions they may belong. Thus all the islands scattered through the Mare Australe and the Mare

* Pyrois I take to be some terrestrial region, although I have not been able to find any translation of the name.—*Tr.*

Erythraeum belong to this category, so too the long peninsula called Deucalionis Regio and Pyrrhæ Regio, and in the vicinity of the Mare Acidalium the regions designated by the names of Baltia and Nerigos. The most natural idea, and the one to which we should be led by analogy, is to suppose these regions to represent huge swamps, in which the variation in depth of the water produces the diversity of colors. Yellow would predominate in those parts where the depth of the liquid layer was reduced to little or nothing, and brown, more or less dark, in these places where the water was sufficiently deep to absorb more light, and to render the botton more or less invisible. That the water of the sea, or any other deep and transparent water, seen from above, appears more dark the greater the depth of the liquid stratum, and that the land in comparison with it appears bright under the solar illumination, is known and confirmed by certain physical reasons. The traveller in the Alps often has occasion to convince himself of it, seeing from the summits, the deep lakes with which the region is strewn, extending under his feet as black as ink, whilst in contrast with them even the blackest rocks illumined by the sunlight appeared brilliant.*

Not without reason then have we hitherto attributed to the dark spots of Mars the part of seas, and that of continents to the reddish areas which occupy nearly two-thirds of all the planet, and we shall find later, other reasons which confirm this method of reasoning. The continents form in the northern hemisphere a nearly continuous mass, the only important exception being the great lake called the Mare Acidalium, of which the extent may vary according to the time, and which is connected in some way with the inundations which we have said were produced by the melting of the snow surrounding the north pole. To the system of the Mare Acidalium undoubtedly belong the temporary lake called Lacus Hyperboreus and the Lacus Niliacus. This last is ordinarily separated from the Mare Acidalium by means of an isthmus or regular dam, of which the continuity was only seen to be broken once for a short time in 1888. Other smaller dark spots are found here and there in the continental area, which we may designate as lakes, but they are certainly not permanent lakes like ours, but are variable in appearance and size according to the seasons, to the point of wholly disappearing under certain

* This observation of the dark color which deep water exhibits when seen from above, is found already noted by the first author of antique memory, for in the Iliad (verses 770-71 of book V) it is described how "the sentinel from the high sentry-box extends his glance over the *wine-colored* sea, *οἴνοπα πόντον*." In the version of Monti the adjective indicating the color is lost.

circumstances. Ismenius Lacus, Lunæ Lacus, Trivium Charontis and Propontis are the most conspicuous and durable ones. There are also smaller ones, such as Lacus Moeris and Fons Juventae which at their maximum size do not exceed 100 to 150 kilometers (60 to 90 miles) in diameter, and are among the most difficult objects upon the planet.

All the vast extent of the continents is furrowed upon every side by a network of numerous lines or fine stripes of a more or less pronounced dark color, whose aspect is very variable. These traverse the planet for long distances in regular lines, that do not at all resemble the winding courses of our streams. Some of the shorter ones do not reach 500 kilometers (300 miles), others on the other hand extend for many thousands, occupying a quarter or sometimes even a third of a circumference of the planet. Some of these are very easy to see, especially that one which is near the extreme left-hand limit of our map, and is designated by the name of Nilosyrtis. Others in turn are extremely difficult, and resemble the finest thread of spider's web drawn across the disc. They are subject also to great variations in their breadth, which may reach 200 or even 300 kilometers (120 to 180 miles) for the Nilosyrtis, whilst some are scarcely 30 kilometers (18 miles) broad.

These lines or stripes are the famous canals of Mars, of which so much has been said. As far as we have been able to observe them hitherto, they are certainly fixed configurations upon the planet. The Nilosyrtis has been seen in that place for nearly one hundred years, and some of the others for at least thirty years. Their length and arrangement are constant, or vary only between very narrow limits. Each of them always begins and ends between the same regions. But their appearance and their degree of visibility vary greatly, for all of them, from one opposition to another, and even from one week to another, and these variations do not take place simultaneously and according to the same laws for all, but in most cases happen apparently capriciously, or at least according to laws not sufficiently simple for us to be able to unravel. Often one or more become indistinct, or even wholly invisible, whilst others in their vicinity increase to the point of becoming conspicuous even in telescopes of moderate power. The first of our maps shows all those that have been seen in a long series of observations. This does not at all correspond to the

appearance of Mars at any given period, because generally only a few are visible at once.*

Every canal (for now we shall so call them), opens at its ends either into a sea, or into a lake, or into another canal, or else into the intersection of several other canals. None of them have yet been seen cut off in the middle of the continent, remaining without beginning or without end. This fact is of the highest importance. The canals may intersect among themselves at all possible angles, but by preference they converge towards the small spots to which we have given the name of lakes. For example, seven are seen to converge in Lacus Phœnicis, eight in Trivium Charontis, six in Lunæ Lacus and six in Ismenius Lacus.

The normal appearance of a canal is that of a nearly uniform stripe, black, or at least of a dark color, similar to that of the seas, in which the regularity of its general course does not exclude small variations in its breadth, and small sinuosities in its two sides. Often it happens that such a dark line opening out upon the sea is enlarged into the form of a trumpet, forming a huge bay, similar to the estuaries of certain terrestrial streams. The Margaritifer Sinus, the Aonius Sinus, the Auroræ Sinus, and the two horns of the Sabaeus Sinus are thus formed, at the mouths of one or more canals, opening into the Mare Erythraeum or into the Mare Australe. The largest example of such a gulf is the Syrtis Major, formed by the vast mouth of the Nilosyrtis, so called. This gulf is not less than 1,800 kilometers (1100 miles) in breadth, and attains nearly the same depth in a longitudinal direction. Its surface is little less than that of the Bay of Bengal. In this case we see clearly the dark surface of the sea continued without apparent interruption into that of the canal. In as much as the surfaces called seas are truly a liquid expanse, we cannot doubt that the canals are a simple prolongation of them, crossing the yellow areas or continents.

Of the remainder, that the lines called canals are truly great

* In a foot note the author refers to a drawing of Mars made by himself, September 15, 1892, and says " * * * At the top of the disc the Mare Erythraeum and the Mare Australe appear divided by a great curved peninsula, shaped like a sickle, producing an ususual appearance in the area called Deucalionis Regio, which was prolonged that year so as to reach the islands ol Noachis and Argyre. This region forms with them a continuous whole, but with faint traces of separation occurring here and there in a length of nearly 6000 kilometers (4000 miles). Its color, much less brilliant than that of the continents, was a mixture of their yellow with the brownish grey of the neighboring seas." The interesting feature of this note is the remark that it was an unusual appearance, the region referred to being that in which the central branch of the fork of the Y appeared. Since no such branch was conspicuously visible this year, it would therefore seem, from the above, that it was the opposition of 1892 that was peculiar, and not the present one.—*Tr.*

furrows or depressions in the surface of the planet, destined for the passage of the liquid mass, and constituting for it a true hydrographic system, is demonstrated by the phenomena which are observed during the melting of the northern snows. We have already remarked that at the time of melting they appeared surrounded by a dark zone, forming a species of temporary sea. At that time the canals of the surrounding region become blacker and wider, increasing to the point of converting, at a certain time, all of the yellow region comprised between the edge of the snow and the parallel of 60° north latitude, into numerous islands of small extent. Such a state of things does not cease, until the snow, reduced to its minimum area, ceases to melt. Then the breadth of the canals diminishes, the temporary sea disappears, and the yellow region again returns to its former area. The different phases of these vast phenomena are renewed at each return of the seasons, and we have been able to observe them in all their particulars very easily during the oppositions of 1882, 1884 and 1886, when the planet presented its northern pole to terrestrial spectators. The most natural and the most simple interpretation is that to which we have referred, of a great inundation produced by the melting of the snows,—it is entirely logical, and is sustained by evident analogy with terrestrial phenomena. We conclude therefore that the canals are such in fact, and not only in name. The network formed by these was probably determined in its origin in the geological state of the planet, and has come to be slowly elaborated in the course of centuries. It is not necessary to suppose them the work of intelligent beings, and notwithstanding the almost geometrical appearance of all of their system, we are now inclined to believe them to be produced by the evolution of the planet, just as on the Earth we have the English Channel and the Chanel of Mozambique.

It would be a problem not less curious than complicated and difficult, to study the system of this immense stream of water, upon which perhaps depends principally the organic life upon the planet, if organic life is found there. The variations of their appearance demonstrated that this system is not constant. When they become displaced, or their outlines become doubtful and ill defined, it is fair to suppose that the water is getting low, or is even entirely dried up. Then in place of the canal there remains either nothing, or at most a stripe of yellowish color differing little from the surrounding background. Sometimes they take on a nebulous appearance, for which at present it is not possible to

assign a reason. At other times true enlargements are produced, expanding to 100, 200 or more kilometers (60 to 120 miles) in breadth, and this sometimes happens for canals very far from the north pole, according to laws which are unknown. This has occurred in Hydaspes in 1864, in Simois in 1879, in Ackeron in 1884, and in Triton in 1888. The diligent and minute study of the transformations of each canal may lead later to a knowledge of the cause of these facts.

But the most surprising phenomenon pertaining to the canals of Mars is their gemination, which seems to be produced principally in the months which precede, and in those which follow the great northern inundation, at about the times of the equinoxes. In consequence of a rapid process, which certainly lasts at most a few days, or even perhaps only a few hours, and of which it has not yet been possible to determine the particulars with certainty, a given canal changes its appearance, and is found transformed through all its length, into two lines or uniform stripes, more or less parallel to one another, and which run straight and equal with the exact geometrical precision of the two rails of a railroad. But this exact course is the only point of resemblance with the rails, because in dimensions there is no comparison possible, as it is easy to imagine. The two lines follow very nearly the direction of the original canal, and end in the place where it ended. One of these is often superposed as exactly as possible upon the former line, the other being drawn anew, but in this case the original line loses all the small irregularities and curvature that it may have originally possessed. But it also happens that both the lines may occupy opposite sides of the former canal, and be located upon entirely new ground. The distance between the two lines differs in different geminations, and varies from 600 kilometers (360 miles) and more, down to the smallest limit at which two lines may appear separated in large visual telescopes—less than an interval of 50 kilometers (30 miles). The breadth of the stripes themselves may range from the limit of visibility, which we may suppose to be 30 kilometers (18 miles), up to more than 100 kilometers (60 miles). The color of the two lines varies from black to a light red, which can hardly be distinguished from the general yellow background of the continental surface. The space between is for the most part yellow, but in many cases appears whitish. The gemination is not necessarily confined only to the canals, but tends to be produced also in the lakes. Often one of these is seen transformed into two short, broad, dark lines parallel to one another, and traversed by a yellow line. In these cases

the gemination is naturally short, and does not exceed the limits of the original lake.

The gemination is not shown by all at the same time, but when the season is at hand, it begins to be produced here and there, in an isolated irregular manner, or at least without any easily recognizable order. In many canals, (such as the Nilosyrtis for example), the gemination is lacking entirely, or is scarcely visible. After having lasted for some months, the markings fade out gradually and disappear, until another season equally favorable for their formation. Thus it happens that in certain other seasons, (especially near the southern solstice of the planet), that few are seen, or even none at all. In different oppositions the gemination of the same canal may present different appearances, as to width, intensity and arrangement of the two stripes, also in some cases the direction of the lines may vary, although by the smallest quantity, but still deviating by a small amount from the canal with which they are directly associated. From this important fact it is immediately understood that the gemination can not be a fixed formation upon the surface of Mars, and of a geographical character like the canals. The second of our maps will give an approximate idea of the appearance which these singular formations present. It contains all the geminations observed since 1882 up to the present time. In examining it, it is necessary to bear in mind that not all of these appearances were simultaneous, and consequently that the map does not represent the condition of Mars at any given period, it is only a sort of topographical register of the observations made at different times of this phenomenon.*

The observation of the geminations is one of the greatest difficulty, and can only be made by an eye well practiced in such work, added to a telescope of accurate construction, and of great power. This explains why it is that it was not seen before 1882. In the ten years that have transpired since that time, it has been seen and described at eight or ten observatories. Nevertheless, some still deny that these phenomena are real, and tax with illusion (or even imposture) those who declare that they have observed it.

Their singular aspect, and their being drawn with absolute geometrical precision, as if they were the work of rule or compass, has led some to see in them the work of intelligent beings, inhabitants of the planet. I am very careful not to combat this

* This map may be found also in "La Planète Mars" by Flammarion, p. 440. —*Tr.*

supposition, which includes nothing impossible. (To mi guarderò bene dal combattere questa supposizione, la quale nulla include d' impossibile). But it will be noticed that in any case the gemination cannot be a work of permanent character, it being certain that in a given instance it may change its appearance and dimensions from one season to another. If we should assume such a work, a certain variabillty would not be excluded from it for example, extensive agricultural labor and irrigation upon a large scale. Let us add further that the intervention of intelligent beings might explain the geometrical appearance of the gemination, but it is not at all necessary for such a purpose. The geometry of nature is manifested in many other facts, from which are excluded the idea of any artificial labor whatever. The perfect spheroids of the heavenly bodies and the ring of Saturn were not constructed in a turning lathe, and not with compasses has Iris described within the clouds her beautiful and regular arch. And what shall we say of the infinite variety of those exquisite and regular polyhedrons in which the world of crystals is so rich! In the organic world, also, is not that geometry most wonderful which presides over the distribution of the foliage upon certain plants, which orders the nearly symmetrical, starlike figures of the flowers of the field, as well as of the animals of the sea, and which produces in the shell such an exquisite conical spiral, that excels the most beautiful master-pieces of gothic architecture? In all these objects the geometrical form is the simple and necessary consequence of the principles and laws which govern the physical and physiological world. That these principles and these laws are but an indication of a higher intelligent power, we may admit, but this has nothing to do with the present argument.

Having regard then to the principle that in the explanation of natural phenomena it is universally agreed to begin with the simplest suppositions, the first hypotheses on the nature and cause of the geminations have for the most part put in operation only the laws of inorganic nature. Thus, the gemination is supposed to be due either to the effects of light in the atmosphere of Mars, or to optical illusions produced by vapors in various manners, or to glacial phenomena of a perpetual winter, to which it is known all the planets will be condemned, or to double cracks in its surface, or to single cracks of which the images are doubled by the effect of smoke issuing in long lines and blown laterally by the wind. The examination of these ingenious suppositions leads us to conclude that none of them seem to correspond entirely with

the observed facts, either in whole or in part. Some of these hypotheses would not have been proposed, had their authors been able to examine the geminations with their own eyes. Since some of these may ask me directly,—Can you suggest anything better? I must reply candidly, No.

It would be far more easy if we were willing to introduce the forces pertaining to organic nature. Here the field of plausible supposition is immense, being capable of making an infinite number of combinations capable of satisfying the appearances even with the smallest and simplest means. Changes of vegetation over a vast area, and the production of animals, also very small, but in enormous multitudes, may well be rendered visible at such a distance. An observer placed in the Moon would be able to see such an appearance at the times in which agricultural operations are carried out upon one vast plain,—the seed time and the gathering of the harvest. In such a manner also would the flowers of the plants of the great steppes of Europe and Asia be rendered visible at the distance of Mars,—by a variety of coloring. A similar system of operations produced in that planet may thus certainly be rendered visible to us. But how difficult for the Lunarians and the Areans to be able to imagine the true causes of such changes of appearance, without having first at least some superficial knowledge of terrestrial nature! So also for us, who know so little of the physical state of Mars, and nothing of its organic world, the great liberty of possible supposition renders arbitrary all explanations of this sort, and constitutes the gravest obstacle to the acquisition of well founded notions. All that we may hope is that with time the uncertainty of the problem will gradually diminish, demonstrating, if not what the geminations are, at least what they can not be. We may also confide a little in what Galileo called "the courtesy of Nature," thanks to which, sometime from an unexpected source, a ray of light will illuminate an investigation at first believed inaccessible to our speculations, and of which we have a beautiful example in celestial chemistry. Let us therefore hope and study.

A SIMPLE METHOD OF MOUNTING AN EQUATORIAL AXIS ON BALL BEARINGS.

F. L. O. WADSWORTH.

The usual method of mounting an equatorial axis on ball bearings is to use a single, or if the instrument is very heavy, a double

row of live steel balls or rolls at each end of the axis to receive the component of pressure perpendicular to it with a third row at the lower end to take the end thrust. The components of pressure on these two sets of bearings respectively are $w \cos \varphi$ and $w \sin \varphi$; where w is the total weight of rotating parts and φ the latitude of the place. The total pressure acting to produce friction is therefore $Pw(\sin \varphi + \cos \varphi)$ which is greater than the actual weight to be supported in the ratio,

$$\frac{\sin \varphi + \cos \varphi}{1}.$$

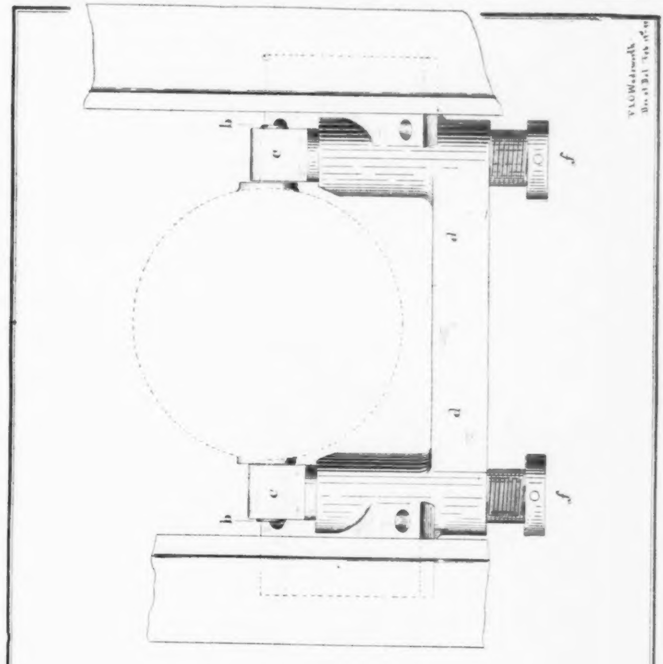
It is a maximum for $\varphi = 45°$ when $\sin \varphi + \cos \varphi = \sqrt{2}$ or $P = 1.414w$.

This method of support is illogical in another respect, for when the latitude is greater than 30° this third row of balls has to support more than ½ the weight of the axis, and the superficial pressure is therefore from twice to three times as great as on the balls of the lateral bearings, in which the pressure is distributed over two or perhaps over four rows. It is true, that wear in the end thrust bearing is of less importance than in the lateral bearings, because the former will not alter the adjustment of the axis, while the latter may; but good mechanical design would aim at making the conditions of wear as nearly uniform as possible in all directions.

To secure this result and to avoid the other disadvantages, just pointed out, of the ordinary method of mounting we may use, instead of the customary three rows of balls, a single row, so placed that the plane of the bearing intersects the axis of the equatorial in the center of gravity of the latter. If this condition is fulfilled and the ball races properly designed to resist a vertical pressure, (as in Fig. 1, Plate I), the one row will support the axis in equilibrium at any angle, while allowing perfect freedom of rotation. In practice the outer race or ball cap is supported on a pair of spiral springs as shown in Plate II, which is a scale drawing of a ball bearing, recently designed on the above principle for the support of the equatorial axis of a 20-in. Foucault Sideristat. The axis is hollow, and as no telescope is attached to it, it is comparatively light, and the steel balls are therefore only ½ inch in diameter. The outer collar *a* is provided with two diametrically opposite trunnions *b*, *b*, which rest in the forked ends of two vertical rods *c*, *c*. The whole weight is carried by the two spiral steel springs whose tension can be adjusted by means of the screws *f*, *f*.

The casting *d*, *d*, which carries the screws and springs is bolted

A Simple Method of Mounting an Equatorial Axis on Ball Bearings.



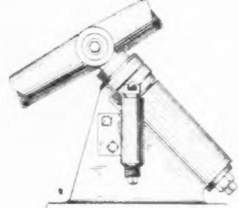


Fig 2

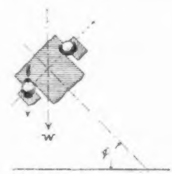


Fig 1

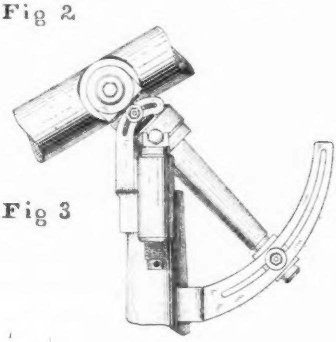


Fig 3

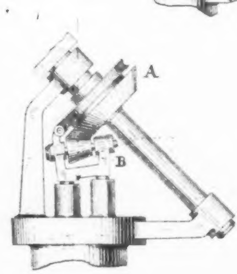


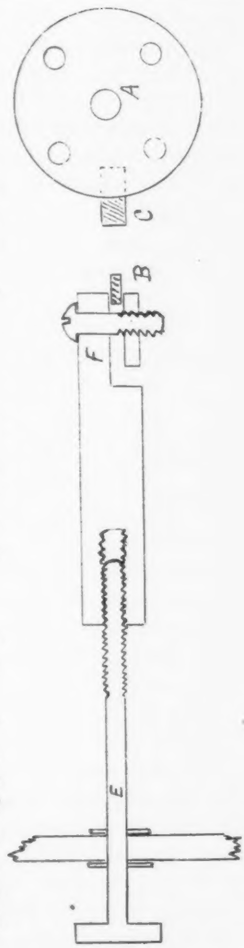
Fig 4

to the sides of the box-shaped frame which carries the axis and clock-work.

At each end of the axis is a plain parallel bearing which serves to steady it and preserve the alignment, but which carries none of the weight.

The central ball-bearing is, as will be readily seen from the drawing, self-adjusting for wear and by simply loosening the bolts *n*, *n*, the outer collar *a* may be removed and an entire new set of balls substituted whenever necessary.

Another advantage of this method of mounting is that it may be applied, at small expense, to old instruments whose axes are already mounted in plain bearings. If the support for the axis is of the usual solid form, it will be necessary to cut away a small portion of it just below the center of gravity of the system (as in Fig. 2); but no returning of the axis itself is necessary.



For portable instruments, the form of support shown in Fig. 3 may be adopted which allows the instrument to be readily adjusted for any latitude. As the pressure on the end boxes is very slight the dotted sectors to which they are bolted need only be heavy enough to steady the axis and prevent vibration, all the weight as before being carried by the central row of balls which is placed directly above the central pillar.

* * * * * * * *

A second method of support which has more recently suggested itself is shown in Fig. 4.

Here the axis does not rest immediately upon balls, but has turned upon it a conical wheel A, whose face makes with the axis an angle, φ, equal to the latitude of the place. This wheel is in such a position on the axis that when the latter is properly mounted, the perpendicular through the center of gravity of the system, passes through the center of the lower face.

It rolls upon a second smaller conical wheel, B, whose axis is mounted on balls, and supported on spiral springs as before. The ball races are designed to take the slight end thrust, due to the resolved component of the weight, along the axis of B, which component will be equal to

$$w \frac{b}{a} \sin \varphi = P$$

Hence if $\varphi = 45^\circ$ and $b = \frac{1}{3}a$, $P = \frac{1}{4}w$. The end thrust is there-

fore a comparatively small part of the whole and may be still farther reduced by making the wheel B smaller. For the sake of steadiness it might be better to use instead of a single wheel, B, placed directly under the axis, two, placed one on each side of the vertical, after the manner of ordinary friction wheels. With this method of construction the worm wheel could be placed immediately alongside the wheel A, or even cut upon an extension of the wheel itself, and the driving worm mounted on the same carriage which supports the axes of the friction rolls.

WASHINGTON, D. C., Feb. 1894.

RECENT OBSERVATIONS OF THE SATELLITES OF JUPITER.*

WILLIAM H. PICKERING.‡

The following observations were made at the Lowell Observatory, and have, of course, no connection with the Arequipa work, although made by the same observer. Two years ago I first described the peculiar phenomena which the satellites of Jupiter present, phenomena which are characteristic of them, and which distinguish them from all other bodies in the solar system as far as we are at present aware. Yet apparently these observations have been confirmed at no other Observatory up to the present time. At first sight this may seem extraordinary, but in point of fact it merely comfirms what has already been said of the great advantages for astronomical work which a desert climate possesses, advantages whose magnitude would scarcely be credited by those who have not had an opportunity to test it for themselves. In selecting the location of the Lowell Observatory the point especially insisted on was that the climate should be extremely dry, and the soil arid. Thanks to its location, the "seeing" is seldom as bad as that which is common in moister climates, and this it seems to me is the reason why others have failed to confirm what is apparently perfectly evident here. Thus, from September 9, when the observations began, until the end of the month, when the present series closed, there were but four nights when I was unable to see one or more of these bodies length ened or shortened, as the case might be, in the manner peculiar to itself,—and two of these nights were cloudy. It is in part to describe some of these newly observed phenomena, and in part to

† Communicated by the author.

‡ This article and the following one were written for *Astronomy and Astro-Physics*. They are both so important and timely that we give them place in this publication also.—*Ed.*

aid others to see the more easily detected features, that the present article has been prepared.

To facilitate my work I have constructed what may be called a scale of ellipticities. Twelve small ellipses of varying excentricity were drawn upon white paper. They were then cut out with all possible accuracy, and pasted upon a piece of black cardboard. The vertical axis of each ellipse measures 10 millimeters. The lengths of the horizontal axes are indicated by the following numbers, which also indicate the arrangement of the ellipses upon the cardboard:

9.6	12.4	12.8
10.0	12.0	13.2
10.4	11.6	13.6
10.8	11.2	14.0

It will be seen that the horizontal diameter of each ellipse exceeds that of its predecessor by 0.4 millimeters. I have found by trial that by looking alternately at a given satellite and at the scale, that even under unfavorable conditions one may generally be certain which of these ellipses the satellite most nearly resembles. If the satellite is well seen, however, one can mentally divide each of these scale divisions into four parts, and thus determine the ellipticity of the disc to within one per cent of the minor axis. The four longest ellipses are only used for studying occultations and eclipses, the discs of the satellites themselves never reaching such a high ellipticity when unaffected by their primary. It has been found that with a power of 1200 diameters the satellites usually appear more nearly round by about two per cent than with a power of 800, a difference for which we must always make allowance when comparing observations made with these different magnifications. It is most important in delicate observations of this sort, that the line joining the two eyes should always hold the same position relatively to the major axis of the disc of the satellite. If placed alternately parallel and perpendicular to it, both the position-angle and the apparent ellipticity will be found to vary owing to astigmatism from which no body is entirely free. In my own case the ellipticity of the 1st satellite is about four per cent greater when the line joining my eyes is perpendicular to the major-axis, than when it is parallel to it. The position-angle is also probably shifted through about 5°.

As it would seem that some astronomers are still doubtful about the accuracy of my observations made upon these bodies in 1892, I will describe one made upon September 18 of the present year, which is, I think, of interest for several reasons. At

$16^h\ 34^m$ the 18-inch Brashear telescope showed that the 1st and 3d satellites were very near together, and that both of them presented elongated discs, the elongations being nearly at right angles to one another. The 2d and 4th satellites were also slightly elongated, but in different directions. The 12-inch Clark telescope was then turned upon the planet, and the observation repeated without difficulty. I next went to the 6-inch Clark, and was surprised to find that with a power of 400, not only the elongations of the 1st and 3d were easily seen, but even that of the 4th could be detected. I especially noted that the elongations had precisely the same position angles relatively to each other as in the larger instruments. The seeing was very good at the time,—8 on a scale of 10.

Since the elongations occurred in different directions for the different satellites, the appearance clearly could not be due to atmospheric conditions, or to the eye. Since identically the same appearance was seen in three different telescopes, it could not be instrumental; and finally, since the elongation has been seen at different times by at least half a dozen different persons, it cannot be a personal idiosyncrasy.

Besides furnishing another proof of the genuineness of the phenomenon, I think this observation has two other important bearings. First, as showing the relative importance of atmosphere *versus* aperture, for delicate visual observations of this sort. In this same category would be included also studies of planetary surface detail, as distinguished from the examination of very faint objects. In other words, if an observer wishes to study very faint stars he must have a large telescope. If he wishes to study the neighboring planets and brighter satellites he may use a small telescope, but he must have a very good atmosphere.

Secondly, it is hoped that this observation may encourage possessors of small telescopes to try them on the satellites, and it is possible that they may succeed in verifying what larger telescopes less favorably located have heretofore failed to detect. The test of the adequacy of the instrument and atmosphere is a simple one; it is merely, will the disc of the satellite satisfactorily bear a magnification of 400 diameters? If so, then under the most favorable conditions the ellipticity of the discs of the three larger ones should be capable of detection.

In order to aid those who may feel inclined to pursue this investigation, I have prepared ephemerides for the 1st and 3d satellites. They will be found at the end of this article. Until others have succeeded in perceiving the varying elongations of these

bodies, it is evidently useless to predict the shapes for the 2d and 4th, which are much more difficult objects to study.

Returning now to this year's work, upon September 21, an opportunity was offered to repeat my former observation upon the occultation of the 3d satellite behind the dark limb of Jupiter. A very satisfactory series of micrometer measurements were obtained for the determination of the extent and density of Jupiter's atmosphere. The flattening of the satellite due to refraction, as it approached the dark limb of the planet was very evident, and was carefully measured upon the scale of ellipticities. As the satellite set behind the planet, the dark space separating the termination from the dark limb of Jupiter was very marked, and measured about $0''.3$ as determined by comparison with the threads of the micrometer. The cusps of the setting satellite seemed well defined and pointed, but the seeing was unfortunately too poor at the time to permit me to see the illumination of the atmosphere of Jupiter by the satellite after it had set as was done in 1892. (ASTRONOMY AND ASTRO-PHYSICS, 1893, p. 395.)

Surface detail has been clearly seen upon the 1st, 3d, and 4th satellites. Upon the two latter its nature seems to be the same as previously designated. Upon the former, in addition to the dark line seen in 1892, the bright equatorial region so-called, has been clearly recognized upon several occasions, even when the satellite was not in transit. In fact in one or two instances it was so conspicuous and easily visible, that it appeared at first difficult to understand why it had not been detected earlier. Further investigation, however, showed that its form is not that of a belt, as hitherto supposed, but rather that of an elongated bright mass, or possibly two masses, arranged in a plane nearly parallel to the orbit. In certain positions of the satellite these markings are, therefore, very conspicuous, while in others they are invisible. Under the latter conditions the dark line shown in my drawings of 1892 is to be seen. This explains then why I missed the bright regions in my earlier studies of this body, as it was examined for detail upon only a few occasions, and upon those occasions I apparently sketched the satellite's other side! It is thought possible that an independent determination of the period and direction of rotation of this satellite may be obtained by means of these markings, and I hope shortly to undertake this investigation.

In the paper above referred to (ASTRONOMY AND ASTRO-PHYSICS, 1893, p. 392) describing my observations in 1892, two periods were given for the 1st satellite, differing from one another by fifteen seconds, one based on the supposition that the direction of

rotation of the satellite upon its axis was direct, and the other considering the direction of rotation to be retrograde. It was stated at that time that observations made at the next opposition would decide between these two periods, as the difference in the time when the circular phase was presented by the satellite would then have amounted to over three hours. At that opposition, however, I was not so situated that I could make the observations myself, and unfortunately no one else succeded in making them. Now it happens that at the end of two years 1321 retrograde revolutions very nearly equal 1320.5 direct revolutions, and by a curious coincidence, the position of the Earth is such at the present time, that the correction which this factor introduces into the computation almost exactly makes up the dis crepancy between the two results, so that the round phase o the satellite, upon the theory of direct rotation, would occur upon September 30 at $17^h\ 38^m$ M. M. T., and upon the theory of retrogade rotation upon September 30, at $17^h\ 40^m$ M. M. T. It is therefore quite impossible at present to distinguish between the two periods, by this method. It will be noted that this is the first occasion since 1892 when such an unfortunate coincidence could have occurred.

We will now see how this year's observations of the circular phase of the satellite bears out these predictions. In the following table, both the direct and retrograde theories are presented. The figures in the first column give the observations of the circular phase in 105th meridian time; those in the second the theoretical times as deduced from these observations, assuming that the rotation of the satellite is direct; those in the third the difference between the two; while those in the fourth and fifth columns give the theoretical times and differences assuming a retrograde rotation:

SATELLITE I.

Observed.	Direct Rotation.		Retrograde Rotation.	
d h m	h m	m	h m	m
Sept. 9 18 12	18 28	+ 16	18 36	+ 24
15 18 14	18 06	− 8	18 11	− 3
17 16 12	15 47	− 25	15 51	− 21
20 15 40	15 37	− 3	15 37	− 3
21 17 46	17 44	− 2	17 43	− 3
23 15 21	15 25	+ 4	15 23	+ 2
24 17 29	17 32	+ 3	17 29	0
30 16 57	17 10	+ 13	17 04	+ 7
Average deviation		$\pm\ 9^m$		$\pm\ 8^m$

The theory of retrograde rotation best satisfied the observations of 1892. The observations of 1894 indicate a slight advantage in its favor. This advantage would be much more marked, were it not for the first observation, which was made by daylight. The present investigation shows that the circular phase really occurred that morning after the observations had ceased. In a rigorous discussion, it would therefore probably be best that this observation should be discarded, as incomplete. This would reduce the average deviations of an observation on the hypothesis of retrograde rotation to 5^m, and would make the most probable time of the last observation, September 30^d 17^h 07^m.

We have already seen that the theoretical time of the circular phase, based upon the observations made in 1892, was September 30^d 17^h 40^m. It should be stated, moreover, that I purposely delayed making this latter computation until all the observations save the last one had been secured, in order that my mind should be entirely unbiased in the matter. On completing the computations, it was satisfactory to note that the accuracy of the observations made in 1892 was sufficient to enable me to compute the time of the circular phase for two years in advance with an error of only 33^m.

The recent observations show that a correction of $1^s.5$ should be applied to the period formerly published. This period was 13^h 03^m $10^s.8$ upon the hypothesis of retrograde rotation. The corrected period therefore now becomes 13^h 03^m $09^s.3$, and is probably correct within $0^s.2$.

The change in shape of the satellite at the time that the circular phase is assumed is quite marked, and much more rapid than at any other period. The name "circular phase," although apparently quite applicable in 1892, does not appear to be so at the present time, since the observations show that the ellipse, although it becomes much shortened, never quite reaches the circular shape. Whether it will do so later remains to be seen. The following series of figures illustrate the nature of a night's observations, and indicate the length of the major axis of the elliptical disc in terms of the minor one:

SATELLITE I.

Date.				Ell.	Date.				Ell.	Date.				Ell.
	d	h	m			d	h	m			d	h	m	
Sept.	23	15	06	115	Sept	24	16	25	118	Sept.	24	17	29	108
"		"	13	"	"		"	34	114	"		"	37	"
"		"	21	112	"		"	39	"	"		"	39	"
"		"	27	114	"		"	45	112	"		"	48	109
"		"	34	118	"		"	51	"	"		"	52	110
"		"	36	116	"		"	57	"	"		"	58	112
"		"	40	118	"		17	04	110	"		18	04	"
"		"	46	120	"		"	14	109	"		"	11	114
"		"	51	119	"		"	19	108	"		"	29	115
"		16	02	"	"		"	24	"	"		"	33	116

Theory indicates that the circular phase occured upon September 23, at $15^h\ 26^m$, and upon September 24, at $17^h\ 32^m$.

In the computation of an ephemeris for the 1st satellite we must make use of the following data:—

Period, $13^h\ 03^m\ 09.3^s$
Rotation, retrograde.
Epoch, 1894, Sept. $30^d\ 23^h\ 50^m$.

The period indicates the mean solar interval which elapses from one circular phase to the next but one, as seen from the Sun. The following approximation is a very convenient one, 57 periods equal 31 days. This involves an error in the period of only $0^s.17$, which may be neglected for ordinary purposes. We will continue to assume the rotation retrograde, as that assumption continues to accord best with the observations. The epoch represents the Greenwich Mean Time at which the satellite would have appeared circular as seen from the center of the Sun, had the velocity of light been infinite. To compute an ephemeris, we must allow for the time required by light to pass between the satellite and the Earth, and we must also make a correction for the angle between the Sun and the Earth as seen from the satellite. The first correction is always positive, the second is negative before opposition, and positive afterwards.

Since the rotation is retrograde, the sidereal period of the satellite is longer than the solar one. Its length is $13^h\ 03^m\ 15^s.2$. The ephemeris is more quickly computed from its solar period, since the angular distance of the Sun and Earth as seen from Jupiter is given for every other day by Marth, but the sidereal period may be employed if we compute the longitude of Jupiter as seen from the Earth. It is of interest to note that this satellite is one of the

few bodies in our system whose period is now known with considerable accuracy. It ranks in order next after Mars.

The following ephemeris gives the Greenwich Mean Times at which the satellite will present the nearest approach to a circular phase as seen from the Earth. The position angle in general varies from 0° to + 12° with regard to Jupiter's equator, but occasionally exceeds these limits, especially when the disc is near its minimum elongation. This position angle is readily measured, but is subject to rapid fluctuations, whose nature I am investigating at the present time, and hope before long to be able to predict.

Ephemeris for Satellite I.

Nov.	1	06	39	Nov.	13	12	22	Nov.	25	18	07	Dec.	7	23	52
		13	10			18	54		26	00	38		8	06	24
		19	42		14	01	25			07	10			12	55
	2	02	13			07	57			13	42			19	27
		08	45			14	28			20	14		9	01	58
		15	16			21	00		27	02	45			08	30
		21	48		15	03	32			09	17			15	02
	3	04	19			10	04			15	48			21	37
		10	51			16	35			22	20		10	04	05
		17	23			23	07		28	04	52			10	37
		23	55		16	05	38			11	24			17	08
	4	06	26			12	10			17	55			23	40
		12	58			18	42		29	00	27		11	06	12
		19	29		17	01	14			06	58			12	44
		02	01			07	45			13	30			19	15
	5	08	33			14	17			20	02		12	01	47
		15	05			20	48		30	02	34			08	18
		21	36		18	03	20			09	05			14	50
	6	04	08			09	52		30	15	37			21	22
		10	39			16	24			22	08		13	03	54
		17	11			22	55	Dec.	1	04	40			10	25
		23	42		19	05	27			11	12			16	57
	7	06	14			11	58			17	44			23	28
		12	46			18	30		2	00	15		14	06	00
		19	18		20	01	02			06	47			12	32
	8	01	49			07	34			13	18			19	04
		08	21			14	05		2	19	50		15	01	35
		14	52			20	37		3	02	21			08	07
		21	24		21	03	08			8	53			14	39
	9	03	55			09	40			15	25			21	11
		10	27			16	12			21	57		16	03	42
		16	59			22	44		4	04	28		16	10	41
		23	31		22	05	15			11	00			16	46
	10	06	02			11	47			17	32			23	18
		12	34			18	18		5	00	04		17	05	49
		19	05		23	00	50			06	35			12	21
		01	37			07	22			13	07			18	53
	11	08	09			13	54			19	38		18	01	25
		14	41			20	25		6	02	10			07	56
		21	12		24	02	57			8	42		18	14	28
	12	03	44			09	28			15	14			20	59
		10	15			16	00			21	45		19	03	31
		16	47			22	32		7	04	17			10	03
		23	18		25	05	04			10	48			16	35
	13	05	50			11	35			17	20			23	06

Dec.	20	05	38	Dec.	23	05	27	Dec.	26	05	17	Dec.	29	05	06
		12	10			11	59			11	48			11	38
		18	42			18	31			18	20			18	09
	21	01	13		24	01	03		27	00	52		30	00	41
		07	45		24	07	34			07	24			07	13
		14	17			14	06			13	55			13	45
		20	49			20	38			20	27			20	16
	22	03	20		25	03	10		28	02	59		31	02	48
		09	52			09	41			09	31			09	20
		16	24			16	13			16	02			15	52
		22	56			22	45			22	34			22	32

The periodical changes of the 3d satellite are not as yet sufficiently well understood to enable me to give its ephemeries with the same certainty as that of the 1st. It is likely however to be elongated upon or about the following dates:—November 1, 5, 8, 9, 13, 16, 17, 21, 24, 25, and 29. Upon November 5, 13, 21, and 29, itsposition angle will differ materially from the position it will have upon the other dates, possibly at times by as much as 60°.

On account of the comparatively slow motion of this satellite, it will be some weeks before I can connect our present series of observations with those made in 1892. When that is done, however, its motions will be pretty well understood, and it will then be possible to furnish an accurate ephemeris for many months in advance.

LOWELL OBSERVATORY, October 6, 1894.
Flagstaff, Arizona.

THE GREAT RED SPOT AND OTHER MARKINGS ON JUPITER.

E. E. BARNARD.

THE GREAT RED SPOT.

The surface of Jupiter is very strongly marked this opposition by two broad reddish belts, one on each side of the equator, and a broad white belt between them at the equator.

The Great Red Spot is fairly distinct in outline, though quite pale—a feeble red.

The great bay in the south equatorial belt north of the Red Spot is still persistent and well marked.

Following are observations of the central transit of the Great Red Spot and the deduced longitudes (referred to Marth's System II):

		d	h	m			°
1894	Sept.	23	15	46.5	Standard Pacific Time.	Longitude	360.9
	Sept.	30	16	32.8			360.7
	Oct.	7	17	20.0			361.3
	Oct.	14	18	3.0			359.4

From these observations, it is apparent that the spot is very closely following the motion assigned to it by Mr. Marth.

The following end of the spot is quite dark. There are white regions on its surface. The belt south of it seems to be in contact with the spot, if it does not actually overlap it slightly.

Small Black and White Spots in the Northern Hemisphere.

In the northern equatorial red belt is a number of very small black red spots. They are very slightly elongated in an equatorial direction. There are also in this belt some small well defined white spots—similar in size and form to the dark ones. Indeed in certain regions of the belt, these black and white spots alternate with decided regularity.

Immediately opposite the Great Red Spot and near each other lie a couple of these objects--a black and a white spot. These are strikingly well defined and easy of observation.

I have selected these for a series of measures to determine their relative motion.

The measures show that the white spot (which follows) is slowly gaining on the dark one; the distance is diminishing about 0″.05 daily. This will bring them together near the middle of January, 1895.

The conjunction of these spots would be a very interesting and important phenomenon, if they were exactly in the same parallel, since an occultation of one or the other must then occur and the result would show us which is uppermost in the Jovian atmosphere. Unfortunately, however, there will be no occultation as the south edge of the white spot is exactly on the same parallel with the north edge of the dark one. They will perhaps graze in passing; it will therefore be interesting to watch the conjunction if they remain sufficiently permanent.

Following are the measures of the distance between their centers when one or the other was in transit.

		d	h	m			″
1894	Sept.	23	15	48	Standard Pacific Time.	Distance =	5.82
	Sept.	30			Standard Pacific Time.		5.78
	Oct.	7	17	5	Standard Pacific Time.		5.32

Reduced to distance 5.20 these become

	″
Sept. 23	5.67
Sept. 30	5.52
Oct. 7	4.97

The small black spot seems to have approximately the same rotation period as that of the Great Red Spot.

TRANSIT OF THE SMALL WHITE SPOT.

Sept. 23^d 15^h 55^m Standard Pacific Time. Longitude = 366°.0

TRANSIT OF THE SMALL BLACK SPOT.

Oct. 7^d 17^h 0^m.0 Standard Pacific Time. Longitude 349°.3

Estimated transits of this object on

Sept. 23 gave longitude 349.2
Sept. 30 gave longitude 351.0

This and the other black spots, are very similar in appearance and location to the small black spots of 1890 and 1891. See my papers on these objects in *Monthly Notices* R. A. S., vol. LI and vol. LII. The previous spots, however, decreased their longitudes about a quarter of a degree daily.

The customary white spots in the southern hemisphere are as abundant as ever.

These observations have been made while examining the planet with the 36-in. to get early observations of the fifth satellite.

On Sept. 10^d 17^h 1^m.6 with the 12-inch, another of the small black spots was in transit, and its longitude was 108°.8.

The great white equatorial belt has been singularly free of markings of any kind. There are now, however, a few dusky markings appearing in it.

MT. HAMILTON, Oct. 18, 1894.

THE POLAR CAP OF MARS.*

A. E. DOUGLASS.

The recent disappearance of the Martian snow cap renders more interesting its position and size as observed during the week preceding this occurrence. On October 4 the following side (the side toward the Sun) of the cap was noticed to be much brighter than other portions, presenting an appearance similar to that of June last but on a much smaller scale. On October 5, at nearly 19^h, G. M. T., a very narrow dark line was observed dividing the cap into two slightly unequal parts, the following part being the larger and showing a very bright north-following

* Communicated by the author.

edge. Three hours later a determination of its size and position was made. October 7, at about 19^h, G. M. T., the rift or dividing line in the cap was noticed to have a direction *s.-f.* and *n.-f.* and it was estimated that at $19^h\ 42^m$ it would have a direction perpendicular to the nearest part of the limb.

On October 8 the position and size of the snow cap was again taken, but the rift was not observed though it was in a favorable position. At this observation the cap appeared about three times as large in area as on the 5th and 12th of the month. A lower power eyepiece (630 diameters instead of 840) was used in this case alone, which might explain some of the increase in size by irradiation; but the whole change cannot be thus disposed of. On August 19 a comparison had been made between powers 630 and 420 between which the difference in irradiation should be more noticeable, but no disagreement in the size of the snow cap was found. The snow cap seems actually to have been of larger size on October 8.

No further observations were made upon this object until October 12 at $20^h\ 26^m$, G. M. T. when its size and position were obtained but no rift was seen. The seeing, 3 or 4 on a scale of 10, was not sufficiently good to show it had there been one. On October 13 no polar cap was visible nor has any sign of one been seen since up to the present time (October 17). Since this disappearance no part of the region occupied by the snow cap has appeared as bright as regions close to the northern limb. The region seems different in no respect from the preceding and following limbs of the planet.

The results of the observations are here presented:

Position of Snow Cap.		d	h		Computed Distance from Pole.
	Central Oct.	12	20.4	G. M. T.	
Turned 50° toward *p.*	"	8	21.4	"	4°.3 wt. 2
" 83° "	" "	5	21.9	"	5°.0 " 3
	Mean distances from south pole				4°.7
	Longitude				59°.

Size of snow cap:

Oct. 12.9 E. and W. 0″.72 = 140 miles in longitude.
N. and S. 0″.36 = 175 miles in latitude.
(1° = 36.84 miles) area 19,500 square miles.

Oct. 8.9 E. and W. 0″.99 = 193 miles.
N. and S. 0″.72 = 380 miles.
Area 64,500 square miles.
By computation, width in longitude 146 miles.
By computation, width in latitude 128 miles.

Oct. 5.9 E. and W. 0″.81 = 157 miles in latitude.
N. and S. 0″.36 = 220 miles in longitude.
Area 27,900 square miles.

From nearness to the limb E. and W. measures should have about three times the weight of N. and S. measures.

Rift in snow cap.

Oct. 5.8 rift pointed at (equatorial) long. 101.6
Oct. 7.8 rift pointed at (equatorial) long. 92.4

Mean direction of rift toward longitude 97.0 at the equator.

Since Flammarion gives no instance of the complete disappearance of the snow at either pole we may consider the present case to be the first recorded. The smallest minimum given in "La Planète Mars" was observed by Schiaparelli in 1879 at the south pole. The measured diameter of the cap was 3°.8 or 140 miles, or 1.6 times as large as on October 12. Moreover Schiaparelli's minimum occurred 75 days after summer solstice, but he was inclined to attribute nearly half of this size to irradiation and thought 2° or 74 miles an equally probable figure.

Schiaparelli's minimum occurred 75 days after the summer solstice and for about 55 days longer the cap did not reach 10° in diameter. In the present opposition October 12 was but 42 days after the summer solstice and 130 days after the solstice will bring us to January 8, 1895. Therefore, while it is impossible to say whether or not we shall have occasional reappearances of the polar cap, it seems unlikely that it will attain any great size for some months to come.

LOWELL OBSERVATORY, Flagstaff, A. T., Oct. 17, 1894.

MARS.

PERCIVAL LOWELL.

On Sept. 24th an interesting observation was made here by Mr. Douglass of what appears to have been the formation and subsequent dissipation within twenty-four hours of a cloud over the western half of Elysium, that part of it which lies between Galaxias and Hyblæus. Three accompanying drawings of his show what occurred. On Sept. 22d and 23d the area referred to was of about the same brilliancy as the eastern half of the region, but on Sept. 24th he observed the western half much brighter than the eastern, almost as brilliant as the polar cap; on Sept. 25th the western half had faded again and become darker than the other. Their appearances suggest clouds, forming presumably over high ground, since neither Galaxias nor Hyblæus were in any way obscured. On the contrary he found the canals perceptibly darker.

Solis Lacus has shown a longitudinal division. The division detected by Mr. Douglass begins in the Nectar, a light line running along the middle of it, which is continued, much fainter, through the Lake of the Sun. The best seeing is necessary to see this. Under poorer seeing Solis Lacus has appeared to him triple horizontally, an effect caused chiefly by the dark patches that show in his drawing.

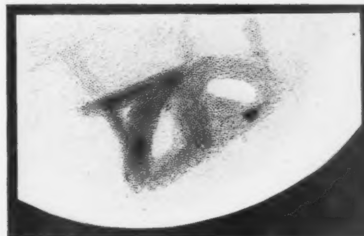
Mr. Douglass finds a small rift in the minute snow-cap which is further interesting as being possibly about the last one to occur before the weather turns cold and the cap begins to increase again.

LOWELL OBSERVATORY, Oct. 12, 1894.

MARS.

PLATE XXXII.

N



N

TRIVIUM CHARONTIS REGION.

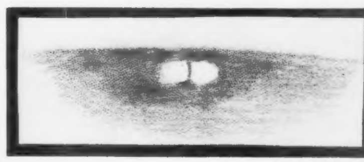
1894, Sept. 22, 19h 20m, G. M. T.
Same as Sept. 23, 20h 43m. Seeing 3 to 5.

1894, Sept. 24, 20h 15m, G. M. T.
Power 630. Seeing 6.

N

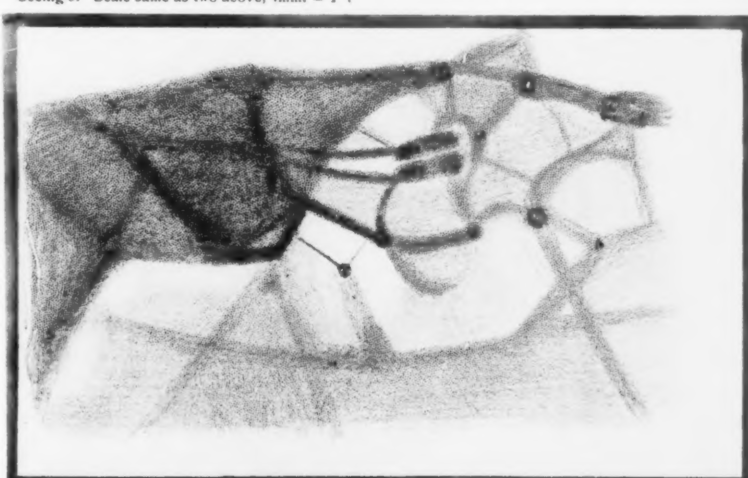
TRIVIUM CHARONTIS REGION.

1894, Sept. 25, 21h 54m, G. M. T. Power 630.
Seeing 3. Scale same as two above, 4mm. = 1″.



S. SNOW.

Oct. 5, 11h 59m, G. M. T. Power 860.
Seeing 7. Scale 20mm. = 1″.



SOLIS LACUS REGION.

1894, Oct. 8, 13h 13m to 14h 05m.
Power 420. Seeing 4 to 9. Scale 4mm. = 1″.

Drawings by A. E. D.

Astro-Physics.

ON A NEW METHOD FOR MAPPING THE SPECTRA OF METALS.

HENRY CREW AND ROBERT TATNALL.

The difference in physical character between the various lines in the spectrum of an element has recently assumed such importance that a table of wave-lengths is now to some extent incomplete unless accompanied by a photographic map. This is especially true for one who is seeking new relations among the wave-lengths.

Thus, in the case of Cadmium, the triplets overlap: but, "owing to the physical similarity of the lines forming any one triplet it is a matter of perfect ease to select them."*

Indeed, in many cases where series have been discovered one might decide to what series a given line belongs quite as well by its appearance as by its wave-length. Rydberg has happily suggested for these series names which describe the appearance of their respective lines.

So far as we are aware, all photographs of metallic spectra which have hitherto been made are with two exceptions, either of spark spectra or spectra of substances vaporized in the carbon arc. The two exceptions to which we refer are, *first*, the well-known spectrum of iron by Kayser and Runge in which the arc employed is that between iron-rods about one centimeter in diameter, and *secondly* a copper arc with which these same gentlemen have attempted to vaporize strontium and thus obtain the triplet† at λ 3800 free from the cyanogen band. They say, however, that the arc worked so badly as to give only one line out of the three.

The well known difficulty with the spark spectrum is that it is almost as characteristic of the slight differences in physical condition under which it is obtained as of the chemical element from which it is obtained. Not only so, but owing to its streaks, as it were, of high temperature ("luminescence"?) there is obtained at the same time with the spectrum of the metal also the spectra of the gases in which the discharge takes place.

In the case of the carbon arc, nature has fortunately grouped its many thousand lines into bands, leaving here and there comparatively clear spaces in which the lines due to substances delib-

* Ames: *Phil. Mag.* July, 1890, p. 45.
† Kayser and Runge: *Wied. Ann. Bd.* 52, p. 115 (1894).

erately introduced into the arc can be studied with a high degree of accuracy as exemplified in the work of Rowland and of Kayser and Runge.

Fortunately, also, in the case of some metals, especially the easily volatile ones, the metallic vapor acts *as if* it shunted off the current from the carbon vapor: and the metal comes out strong in comparison with the carbon.

At the same time, the carbon and cyanogen bands extend practically through the whole spectrum from λ 3500 into the infra red. Not only so, but many of these carbon lines have, as a rule, intensities quite comparable to those of the metallic lines. One ingenious effort has been made by Kayser and Runge (*l. c.*) to rid themselves of the cyanogen bands by working the carbon arc in a current of carbon dioxide. This is partially successful; but, at best, it only *diminishes* the intensity of the band. Messrs. Lewis and Ferry,* speaking of the infra-red metals, say, "It seems as though little more could be done in the discovery of new metallic lines unless the carbon lines are first carefully mapped, or some means is devised for raising the substances investigated to sufficiently high temperature without placing them directly in the [carbon] arc."

We have, therefore, devised and used during the past year the following method for obtaining the arc spectrum of the metallic elements free from carbon, free from air lines, and free from any continuous spectrum.

The idea is simply that of an arc in which one pole rapidly rotates or vibrates, and thus prevents welding and destroys the coating of oxide which in many cases interrupts the current between ordinary metallic poles.

To accomplish this a brass disc is fitted by means of a collar and set screw to the shaft (or counter-shaft) of a small high speed electric motor. Parallel to this brass disc, and upon it as a base, is screwed a similar disc. These discs are used as jaws in which to clamp small pieces of metal to be vaporized. One pole of the electric circuit which includes the arc is connected, by brushes, to the counter-shaft shown in section at A (Fig. 1). The other pole of the arc circuit is connected to another clamp F, which by means of the screw E can be made to approach or recede from the rotating disc. The clamp is also fitted with parallel jaws to receive a small piece of metal, B, to be vaporized. This metal, B, is moved always parallel to itself, and the arc between B and C is maintained always at the same point. Both the rotating and

* *Johns Hopkins University Circular*, May 1894.

the sliding jaws are mounted on the same base with the motor: and the whole is so light as to be easily carried in one hand.

The disc is set in rapid rotation and the metal at B is slowly fed in, by the screw at E, until the arc strikes. The incandescent vapor is then carried out by the disc into the form of an open fan, and is projected upon the slit of the spectroscope by the "image" lens.

In the case of those elements which are easily obtainable in the form of a regulus an entire disc may be made of the metal. With the rarer metals one needs to use only a small piece in the clamp, but the time of exposure is correspondingly lengthened. The disc once started, no attention is required except the feeding in of the metal B. Nearly all the wear is on this piece and very little on the disk, so that the latter will last for a comparatively long time while the former has to be renewed with a frequency depending upon the amount of current employed. We have generally used a hundred-volt circuit and an alternating current of from two to ten amperes. Higher voltages sustain a longer arc and protect the metal from mechanical wear.

For the purpose of a comparison spectrum, is used a second counter-shaft placed parallel to and in the same horizontal plane with the first. This shaft carries an iron disc, about an inch in diameter, against which is fed a piece of iron tubing. The spectrum of any one metal having been photographed, the whole instrument is translated laterally and the current switched on to the iron disc.

While not so convenient as the Sun in many ways, the iron spectrum has an abundance of sharp lines evenly distributed; it permits one to work in all kinds of weather and at night.

The plates whose measures follow will illustrate the method. They were taken with a Rowland concave grating of ten feet radius and ruled with fifty thousand lines. The portion of the plate measured, in each case, covers a part of the spectrum where the carbon bands are strong. Knowing of no adequate method of reproduction, except silver printing, which is too expensive, we have selected three typical plates and simply measured on a dividing engine all the lines visible including "ghosts" and recognized impurities. The tables explain themselves. They include all the lines certainly visible through the reading microscope of the dividing engine; but a still lower power microscope shows a number of weaker lines between those measured.

The wave-lengths were determined not with the highest accuracy possible; but well within a tenth of an Angström unit, which

is usually ample for purposes of identification. The method was interpolation between two of Rowland's standard iron lines, except in the case of copper where, for convenience, the interpolation is between two of Kayser and Runge's copper lines.

PLATE NO. 178. TIN.

Element.	Plate 178.	Kayser and Runge.	Remarks.
Tin	4893.66	3662.44	Third order line, not completely absorbed by glass.
Tin	4762.72	3175.12	Third order line, not completely absorbed by glass.
(Ghost)	4531.20		Second order ghost of Sn 4524.92.
(Ghost)	4528.04		First order ghost of Sn 4524.92.
Tin	4524.91	4524.92	Intensity 2.
(Ghost)	4521.77		First order ghost of Sn 4524.92.
(Ghost)	4518.63		Second order ghost of Sn 4524.92.
	4511.43		Intensity 6: sharp. Not recognized.

PLATE NO. 177. COPPER.

Element.	Plate 177.	Kayser and Runge.	Intensity.	Remarks.
Copper	4003.18	4003.18	5	
	3998.08		6	Hazy.
	3979.97		6	Hazy.
	3976.14		6	Extremely wide and hazy.
Calcium	3968.55		4	Fraunhofer's H.
	3964.27		6	Wide and hazy.
	3961.64		6	Very weak.
	3951.63		6	Very weak.
	3947.00		6	Hazy.
Calcium	3933.76		4	Fraunhofer's K.
	3933.11		6	Wide and hazy.
Copper	3925.36	3925.40	5	
Copper	3821.32	3821.38	5	
Copper	3899.42	3899.43	6	
	3888.73		6	Very weak and hazy: caesium line at λ 3888.83.
	3883.39		6	No resemblance to head of C band at 3883.47.
	3881.75		6	Hazy.
(Ghost)	3865.97			Second order: belongs to Cn. 3860.64.
Copper	3861.90	3861.88	5	
Copper	3860.57	3860.64	2	
Iron	3860.03		6	Fe 3860.03 (K. & R.)
(Ghost)	3857.88		6	First order: belongs to Cn. 3860.64.
	3844.57		6	Wide and hazy.
	3837.48		6	Wide and hazy.
Iron	3825.99		6	Sharp trace of Fe 3826.04 (K. & R.).
Copper	3825.17	3825.13	6	
Copper	3820.97	3821.01	4	
	3820.52		6	Sharp: unlike Hg. 3820.6 (K. & R.).
	3817.57		6	Hazy.
	3813.60		5	Probably not copper.
Copper	3812.08	3812.08	6	Wide and hazy.
Copper	3805.29	3805.33	3	

Element.	Plate 177.	Kayser and Runge.	Intensity.	Remarks.
	3803.64		6	Wide and hazy.
	3800.57		4	Fairly sharp.
	3799.99		5	Rather sharp.
	3797.34		6	Hazy.
	3785.74		6	Wide and hazy.
	3780.20		6	Wide and hazy.
Copper	3771.96	3771.96	4	
	3764.98		6	Wide and hazy.
Copper	3759.56	3759.53	4	
Iron	3758.36		6	Fe 3758.36 (K. & R.).
Iron	3749.61		6	Fe 3749.61 (K. & R.).
	3745.53		5	Hazy.
	3743.53		6	
Copper	3741.36	3841.32	3	
Iron	3737.27		6	Fe 3737.27 (K. & R.).
Iron	3734.96		6	Sharp trace of Fe 3735.00 (K. & R.).
Copper	3734.29	3734.27	3	
	3721.79		6	Hazy.
	3720.89		5	Very sharp.
	3720.09		6	Sharp.
Copper	3712.06	3712.05	4	Hazy.
	3707.31		6	Exceedingly weak and hazy.
Iron	3701.20		6	Trace of Fe 3701.20 (K. & R.).
Copper	3700.61	3700.63	3	
	3699.17		6	Hazy.
	3695.48		6	Hazy.
Copper	3688.38	3688.60	6	Exceedingly wide and hazy.
	3686.67		5	Rather sharp.
	3685.04		6	Rather sharp.
Copper	3684.77	3684.75	3	
Lead	3683.60		6	Faint trace of Pb. 3683.60 (K. & R.).
Copper	3676.96	3676.97	5	
Copper	3672.04	3672.00	5	
Copper	3665.83	3665.85	4	
	3664.21		6	Hazy.
Copper	3659.44	3659.44	5	
Copper	3656.86	3656.90	6	
Copper	3655.99	3655.99	4	
Copper	3654.47	3654.60	6	
Copper	3652.48	3652.56	6	
	3650.97		6	Hazy.
Copper	3648.52	3648.52	5	
Copper	3645.31	3645.32	4	
	3643.80*		6	
Copper	3641.80	3641.79	5	
Copper	3636.01	3636.01	4	
	3632.67		5	Shaded towards violet.
	3629.90		6	
Copper	3627.40	3627.39	4	
Cooper	3624.36	3624.35	5	
Copper	3621.32	3621.33	3	
Copper	3620.47	3620.47	5	
	3619.52		6	Certainly not copper.
(Ghost)	3618.88		6	First order: belongs to Cn. 3621.33.
(Ghost)	3616.37		6	Second order: belongs to Cn. 3621.33.

* The iron line at 3643.80 (K. & R.) is much weaker than some of its neighbors which do *not* show as impurity lines. Hence this line is probably not iron.

Element.	Plate 117.	Kayser and Runge.	Intensity.	Remarks.
Copper	3614.31	3614.31	6	
Copper	3613.85	3613.86	4	
	3610.88		5	Hazy. Strong Cd. line at λ 3610.66.
	3609.43		5	Very sharp.
(Ghost)	3607.22		6	Second order: belongs to 3602.11.
(Ghost)	3604.64		6	First order: belongs to 3602.11.
(Ghost)	3604.30		6	Second order: belongs to 3599.20
Copper	3602.10	3602.11	3	
Copper	3599.20	3599.20	3	

Plate No. 169. Zinc.

Element.	Plate 169.	Kayser and Runge.	Remarks.
(Ghost)	4828.26		Fifth order.
(Ghost)	4824.21		Fourth order.
(Ghost)	4821.19		Third order.
(Ghost)	4817.41		Second order.
(Ghost)	4814.18		First order.
Zinc	4810.79	4810.71	Intensity 1.
(Ghost)	4807.36		First order.
(Ghost)	4804.11		Second order.
(Ghost)	4800.84		Third order.
(Ghost)	4797.30		Fourth order.
(Ghost)	4793.99		Fifth order.
(Ghost)	4738.70		Fifth order.
(Ghost)	4735.52		Fourth order.
(Ghost)	4732.26		Third order.
(Ghost)	4728.89		Second order.
(Ghost)	4725.70		First order.
Zinc	4722.34	4722.26	Intensity 1.
(Ghost)	4719.03		First order.
(Ghost)	4715.79		Second order.
(Ghost)	4712.67		Third order.
(Ghost)	4709.36		Fourth order.
(Ghost)	4705.98		Fifth order.
(Ghost)	4693.27		Fourth order.
(Ghost)	4686.85		Second order.
(Ghost)	4683.50		First order.
Zinc	4680.38	4680.38	Intensity 1.
(Ghost)	4677.04		First order.
(Ghost)	4673.97		Second order.
(Ghost)	4670.66		Third order.
(Ghost)	4667.52		Fourth order.
(Ghost)	4664.25		Fifth order.
Zinc	4630.06	4630.06	Intensity 4.
Zinc	(4613.96)	3075.99	Intensity 6: third order line.
Zinc	(4608.29)	3072.19	Intensity 6: third order line.
Zinc	(4553.83)	3035.93	Intensity 6: third order line.

Out of 98 lines measured on the copper plate it will be noticed that we are unable to identify 41. They are not to be found among Kayser and Runge's values for Ag., Au., Sn., Pb., As., Sb., Mg., Ca., Zn., Sr., Cd., Ba., Hg., Li., Na., K., Rb., Cs., or Fe.

It is probable that these 41 lines belong to impurities whose wave-lengths have not yet been determined (or, at least, not published) with an accurrcy sufficient for identification. It is not impossible, however, that some of these are *new* copper lines. We have found very little difference between "commercial" copper and that which is sold by chemical supply houses under the label "chemically pure."

From the tables it will be seen that the plates are practically clear except for the impurity lines, which are very weak, many of them not showing on a silver print. In any case, a table of impurity lines and ghosts might accompany each map. A few years hence, when the spectra of the metals are more completely measured, such a table will be easily made.

NORTHWESTERN UNIVERSITY,
Evanston, Illinois, July, 1894.

THE INFRA-RED SPECTRA OF METALS.*

E. P. LEWIS AND E. S. FERRY.

In order to extend the range of standards of wave-lengths, we have, under Professor Rowland's direction, been engaged for several months in a bolometric investigation of the infra-red spectra of metals. The approximate wave-lengths of a few lines in this region have been found by Becquerel, Abney, and others. The most recent and reliable results are those obtained by Snow, who, with a fluorspar prism and bolometer, investigated the spectra of the alkaline metals. He measured a number of wave-lengths by an ingenious interference method, with probably the highest degree of precision attainable by the use of a prism. In order to obtain any high degree of accuracy, however, it is necessary to use a concave grating, although the difficulties are very much increased by the greater dispersion and the division of the energy among a number of spectra. On account of these difficulties very little in the way of definite results has as yet been accomplished, because it will be necessary to compare a large number of observations in order to eliminate the many accidental effects which unavoidably occur. We have not as yet succeeded in making a large number of reliable observations, because it has been impossible to take any trustworthy readings during variable weather, there being no means of keeping the room at a constant temperature.

* Johns Hopkins University Circular, No. 112.

Apparatus and Methods of Observation.

The apparatus consists of a six inch concave diffraction grating of 21 ft. 6 in. radius with 14000 lines per inch, mounted on a spectrometer of the usual Rowland pattern,* a bolometer and a galvanometer. The spectrometer is so arranged with heliostat and arc-lamp that by the simple movement of a cord either the solar or arc spectrum can be observed as desired. A salt of the metal whose spectrum is to be investigated is placed in holes drilled in the carbons.

The galvanometer is one designed by Professor Rowland especially for this experiment, and contains several new points that make it much easier to adjust and use than the ordinary forms. The two sides containing the coils are hinged at the bottom, and when closed are fastened to the frame by thumb-screws, enabling one to examine the system with minimum difficulty. The four coils are in brass cups that can be moved to different distances from the needles, or can be entirely removed and others substituted in a few seconds. In these experiments, the coils are wound to about 25 ohms resistance each, and coupled in multiple so that the resistance of the four coils is about 6.25 ohms. For the needless, various kinds of steel were tested, and the selection made by the following device: after the little steel bars were shaped, hardened and magnetized, a pair pointing in the same direction was fastened to a minute glass tube and the period of oscillation observed. The pair giving the most rapid period is the strongest. Then one of the needles was reversed and the period of oscillation again observed. The pair giving the slowest period is the most nearly astatic. The pair of needles best fulfilling these two conditions was selected. The entire system weighs 0.125 grams of which the needles weigh 0.085 grams. Both quartz and silk suspensions have been used, but a silk one has been finally adopted on account of its smaller torsion. The figure of merit of the galvanometer as actually used with a period of ten seconds per single swing is of the order 10^{-9}; but, if desirable, this could be greatly increased. In bolometric work, however, the effect of extraneous disturbances gives a practical limit to the sensitiveness desirable, which, with the apparatus employed under the conditions of this experiment, is about that given above.

Great difficulty was experienced in securing suitable platinum strips for the bolometer. An attempt was made to make strips by electro-deposition on silver, but deposits of a suitable thickness

* For description of this spectrometer see a paper by Dr. J. S. Ames in Astronomy and Astro-Physics, January 1892.

were found too fragile for use. Those finally used were obtained by hammering No. 40 wire flat between two pieces of polished steel, the wire being stretched by a bow of weak spiral spring during the process. The diameter of wire was about 0.042 mm., and the strips produced were about 0.4 mm. in width and 0.004 mm. thick. The platinum was evidently quite impure, its temperature coefficient being only 0.0027. The resistance of the wire was about 2 ohms per inch at 20° C.

The bolometer was arranged differentially according to Professor Rowland's suggestion, so that the effects due to variations in the arc might be to a great extent eliminated. Two parallel strips of platinum about 2.5 cm. long were placed about 4 mm. apart in a vulcanite box containing two openings—one in front to admit the radiation and another in the rear so that the strips could be seen by the observer. The other arms of the bridge were of German silver. Originally each was of fine wire, of about 36 ohms resistance, wound on a brass cylinder screwed in the top of the vulcanite box, but it was found that the current used—about 0.3 amperes—heated it considerably, and the heat, being conducted downward by the brass cylinder, produced considerable disturbance. Arms of larger wire with a resistance of about 4 ohms, wound upon a vulcanite cylinder, were finally substituted with more satisfactory results. It was, of course, impossible to secure a perfect balance on the arms of the bridge, so that it was necessary to put a resistance box as a shunt to an arm. The bolometer was mounted in a brass tube which was substituted for the eye-piece on the spectrometer arm.

The platinum strips being both exposed to the radiation, a double set of readings is obtained on each hot line as the bolometer is moved through the spectrum. There is little danger of confusion arising from the use of two strips, unless a number of strong lines are grouped together, while the diffused radiation from the arc should affect both equally. At first, great trouble and loss of time was experienced from rapid drift of the needle. It seems that nearly all who have used the bolometer have met with this difficulty. Professor Langley, with all the precautions and skillful devices suggested by his long experience, has not succeeded in entirely doing away with it. Snow attributed it to lack of homogeneity in the strips due to smoking over a flame. Our experience indicates that lack of homogeneity is the correct explanation. After breaking the original strips, several pairs of new ones were tried, all giving great trouble. Finally, a pair was used which produced very little drift. Nothing was changed in the

bolometer or its connections except the strips, so it seems evident that the processes of annealing, hammering, soldering and smoking may produce considerable differences in the temperature coefficient of strips from the same piece of wire and to all appearances similar. Thus the selection of suitable strips seems largely a matter of chance.

It was thought best to use rather wide strips on account of the increased sensitiveness due to greater surface exposed to radiation and to the larger current which could be employed. The arc subtended by the strip, however, is less than 12″. This is very near the limits of accurate measurement at present attainable. The strips used by Snow, though very narrow, subtended an angle of about 69″, while of course the dispersion of his prism was much less than that of the grating used in these experiments. The calculated sensitiveness of the bolometer is about .00001 degree.

In order to decrease the effect of air currents about the wrappings of the bolometer, it was decided to try the effect of having all four arms made of thin strips as nearly identical as possible, and having them all close together within the same enclosure. For the purpose of increasing the galvanometer deflections two changes were made in the original arrangement; first, each strip was doubled back upon itself so as to have twice the area and twice the resistance acted upon by the light; and second, all the strips were made effective by placing the ones corresponding to opposite arms of the bridge one back of the other in such a manner that two compound strips were formed each of four times the width of a single one. The same differential arrangement was employed as in the instrument first constructed. The case was made of metal and the openings closed with glass slides so as to be draught tight. A current of 0.3 amperes was used in the bolometer, and for the purpose of keeping the temperature of the instrument constant, this current was kept in the instrument continuously.

The method of observation consists in slowly moving the bolometer through the metallic spectrum produced by the volatilization of a metal in the arc, and noting all galvanometer deflections. The motion of the carriage supporting the bolometer is produced by a very accurate screw made by Professor Rowland's process, enabling the position of the bolometer strips to be read down to .01 of an Angström unit. Similar carbons are then substituted, in which none of the metal has been introduced, and the galvanometer deflections again noted. The deflections observed with the

metal that are not observed when the clean carbons are employed, are due to the bright line spectrum of the metal. Of course, in both sets of observations, there will be many deflections due to extraneous magnetic and mechanical disturbances, but these can usually be distinguished from the deflections produced by a bright line by the fact that with the differential arrangement here employed, a bright line gives four distinct galvanometer deflections of definite directions and separated from each other by definite distances. The only uncertainty occurs when the bright lines are close together, as in the case of a carbon band. To overcome this uncertainty, the region is traversed several times by different bolometers in which the strips are separated from each other by different incommensurable known distances.

As a test of whether the line thus discovered really belongs to the metal to which it is supposed to be due, one strip of the bolometer is placed at the point where the line is supposed to be located and the arc lamp started burning clean carbon. An end of a bored carbon filled with a salt of the metal is then placed in the highly heated gaseous envelope of the poles and the galvanometer observed. If a deflection occurs in the proper direction, the bolometer is moved till the strip is replaced by the other one, and the charged carbon again inserted in the arc. There ought now to occur a deflection of the galvanometer in the opposite direction. By repeating these observations many times, consistent results have already been obtained which locate several infra-red metallic lines. The wave-length of these lines was determined by comparison with the overlapping solar spectrum of the second order.

The region thus far investigated is in the first spectrum and extends only to about 12000 Angström units, so that there is little trouble due to overlapping spectra, which can be cut out by various absorbing media.

The principle difficulty experienced has been with the drifting of the galvanometer needle. After preparing bolometer strips which give a minimum of drift, the precautions which have been found most effective in decreasing it are: thermal insulation of the bolometer and connections in the leads from air draughts, stationary temperature of the strips themselves by the continuous passage of a constant current through them, uniform temperature of the room in which the experiments are performed, frequent astaticization of the galvanometer needles, shielding of the galvanometer connections from radiation by the use of a metallic screen, rheostat for the bolometer shunt having coils of low temperature coefficient.

We at first attempted the investigation of calcium, but the coexistent carbon lines made the attempt hopeless.

It is well known that when sodium and potassium salts are burned in the arc they cool it to such an extent that the carbon lines are almost eliminated in the visible spectrum. For this reason we used sodium, so that complications due to the carbon lines might be avoided, but to our surprise found that scarcely any effect of the kind was noticable in the infra-red. We then decided to confine ourselves in the beginning to the more accurate measurement of lines whose positions are already approximately known. The following infra-red wave-lengths for sodium have been found previously:

Abney.	Kayser and Runge.	Snow.
8199	8200.3	} 8180.
8187	8188.3	

We have found decided indications of lines approximately coincident with the above, but have not yet succeeded in fixing the position of the maxima closely enough for more accurate measurement. Snow found an intense line at 11430, and Kayser and Runge have, by calculation from empirical formula, predicted two lines of wave-lengths 11504.8 and 11481.8. We have found decided evidence of two sodium lines of approximate wave-lengths 11488 and 11468.

We intend to go on with the more accurate measurement of lines whose position is already approximately known, and to make at least a preliminary survey of the infra-red carbon spectrum. It seems as though little more could be done in the discovery of new metallic lines unless the carbon lines are first carefully mapped, or some means is devised for raising the substances investigated to sufficiently high temperature without placing them directly in the arc.

THE SPECTRUM OF MARS.*

W. W. CAMPBELL.

The spectrum of Mars has been observed by several eminent astronomers—Rutherfurd, Secchi, Janssen, Huggins, Vogel, Maunder. These observers had especially in mind the solution of two questions: (*a*) Is there spectroscopic evidence of an *atmosphere* on Mars? and (*b*) Is there spectroscopic evidence of *aqueous vapor* in the atmosphere of Mars?

* *Publications A. S. P.*, No. 37.

The observations made in 1862 in America by Rutherfurd* did not give an affirmative answer to these questions. The meagre results obtained seem to show that his instrumental equipment was not adapted to the solution of this problem.

The details of Janssen's observations have not been published, so far as I know, but his conclusions are expressed in a letter† read to the French Academy in 1867: "I cannot bring this letter to a close without telling you that I have ascended Mt. Etna for the purpose of making some spectroscopic observations which require a great altitude in order to annul the greater part of the influence of our atmosphere. From these observations and from those which I have made at the Observatories of Paris, Marseilles, and Palermo, I believe I can announce to you the presence of aqueous vapor in the atmospheres of Mars and Saturn."

In 1867 Huggins‡ in England observed lines in the spectrum of Mars "apparently coincident with groups of lines which make their appearance when the Sun's light traverses the lower strata of the [Earth's] atmosphere, and which are therefore supposed to be produced by the absorption of gases or vapors existing in our atmosphere. The lines in the spectrum of Mars probably indicate the existence of similar matter in the planet's atmosphere."

We have not the dates and details of Secchi's observations, but in 1872 he wrote§ that he considered his observations "proved the existence of a Martian atmosphere analogous to our own." However, there appears to be a reasonable doubt as to the sufficiency of his observations.

The most extensive observations on this subject are those made by Vogel in Germany in 1873. From his observations he considered that "it is definitely settled that Mars has an atmosphere whose composition does not differ appreciably from ours, and, especially, the Martian atmosphere must be rich in aqueous vapor."¶

Observations by Maunder|| at Greenwich in 1877 confirmed in a general way those made by Vogel.

It is seen that the investigations of five eminent spectroscopists lead them substantially to the same result, viz.: *That Mars' atmosphere is similar to our own.* Their conclusion has been very

* *American Journal Science*, Jan. 1863.
† *Comptes rendus*, vol. LXIV, page 1304.
‡ *Monthly Notices Roy. Ast. Soc.*, vol. XXVII, page 179.
§ *Sugli Spettri prismatici di Corpi celeste*, Rome, 1872.
¶ *Untersuchungen ueber die Spectra der Planeten*, page 20.
|| *Monthly Notices Roy. Ast. Soc.*, vol. XXXVIII, page 35.

generally accepted by astronomers. A careful examination of all the published data has shown me that some of the observations were made under circumstances *extremely unfavorable*, and that between the different sets of observations there is not that close agreement which one would like to see. While I believed that Mars has an atmosphere and that it contains water vapor, it seemed to me that a repetition of the spectroscopic observations under the very favorable circumstances existing here would be valuable.

Among the favorable circumstances we may mention:

(1) Improved spectroscopic apparatus. The observations mentioned above were made from seventeen to thirty years ago, with spectroscopes comparatively crude.

(2) A telescope of great focal-length and correspondingly large aperture. The telescopes used in the early observations were small and short, so that the images of Mars formed by them on the slit-plates would be less than one-third as large as that given by the 36-inch equatorial. This is an enormous advantage, both in estimating the relative intensities of spectral lines, and in comparing the intensities of the centers of the lines (corresponding to the center of Mars' disk) with the intensities of the ends of the same lines (corresponding to the limb of Mars).

(3) The altitude of the Observatory, which eliminates from the problem the absorptive effect of the lower 4200 feet of the Earth's atmosphere, with all its impurities. Most of the old observations were made from near sea-level.

(4) The very dry summer air prevailing here. The average relative humidity is very low at Mt. Hamilton for the months of July and August. In many years it is less than 35 per cent. There is no difficulty in selecting nights for observing the spectrum of Mars when our relative humidity is not more than 20; quite frequently it is less than 20. This is a very important factor, since in examining Mars' spectrum for evidences of aqueous vapor it is very important, as Janssen pointed out in 1867, that we eliminate as far as possible the effect of aqueous vapor in our own atmosphere. The observers do not seem to have taken this factor into account (except Janssen, the details of whose observations appear to be unpublished). By examining the contemporary weather data, I find that some of the observations were made when the relative humidity was 81, 85 and even 90. All the principal published observations were made where the average relative humidity at those seasons of the year is something like 80.

(5) The southern location of the Observatory and the north declination of Mars permit the observations to be made when the planet's altitude is as great as 59°. At an altitude of 59°, the light from Mars passes through 1.17 times as much atmosphere as it would if the planet were in the zenith. The most important of the published observations were made when the planet's altitude was only from 21° to 26°. That is, its light passed through from 2.75 to 2.28 times as much of our atmosphere as it would had the planet been in the zenith! While the observers sought to eliminate the effect of our atmosphere and its aqueous vapor by observing the lunar spectrum when the Moon was at the same altitudes, it must be evident that the Martian spectrum was observed at a tremendous disadvantage. One observation was made, for instance, when the altitude of Mars was only 24° and the relative humidity of our own atmosphere was 85. The effects of any possible Martian atmosphere would be pretty thoroughly drowned by the effects of the great thickness of our own atmosphere, nearly saturated with moisture.

(6) Finally, we may mention that our knowledge of the spectrum of our own atmosphere has been largely increased in the last few years. Thollon's excellent maps, for instance, are of great assistance in this problem.

With all these favorable circumstances, I expected that a confirmation of previous results would be a simple and easy matter.

We shall now state briefly the elements which enter into this problem.

We know by observation that the hemisphere of Mars which is turned toward the Sun is bright, and that the hemisphere which is opposite the Sun is dark. The planet, therefore, shines by reflected sunlight. The spectrum of Mars must be identical with that of the Sun, except as it is modified by the planet's (supposed) atmosphere.

The highly heated interior of the Sun, constituting its most dense portions, radiates light of all possible wave-lengths: that is, its spectrum is a strictly *continuous* band—not crossed by dark lines. The outer portions of the Sun are gaseous, of very much lower temperature than the inner portions, and made up of the vapors of the chemical elements contained in the Sun. These vapors, mostly those of hydrogen and the metals, constitute a sort of *solar atmosphere*. The light radiated from the hotter interior of the Sun does not pass freely through this surrounding atmosphere. It absorbs some of the rays of every wave-length (but more especially the blue and violet rays). This is called a

general absorption. It also selects light of *particular* wavelengths and absorbs that light very strongly, producing the dark lines. The absorption which produces the dark lines is called *selective*, and the lines are called *metallic* lines. The solar spectrum consists of the *continuous spectrum* of the *Sun's interior* modified or interrupted by thousands of (dark) *metallic lines* caused by the *solar atmosphere*.

Our own atmosphere modifies the solar light which passes through it. It exercises both a *general* absorption, which weakens the continuous spectrum, and a *selective* absorption, which introduces at least 1200 additional dark lines. These dark lines —called *telluric* lines—constitute what we may term the spectrum of our atmosphere.

If the planet Mars is surrounded by an atmosphere, it no doubt exercises an absorption upon the solar light which enters it. The rays of light coming to us from the planet originate in the Sun; they pass once through the solar atmosphere; they enter Mars' atmosphere, are reflected partly by the planet's surface and partly by the inner strata of its atmosphere, and then pass out through its atmosphere; and they finally reach us by passing once through our atmosphere. The spectrum of Mars is, therefore, the combined spectrum of the solar, Martian and terrestrial atmospheres. If it *has* no appreciable atmosphere, the spectrum of the planet is simply the combined spectrum of the solar and terrestrial atmospheres.

The problem before us would be practically insoluble if we did not have a convenient means of eliminating the solar and terrestrial spectra, and leaving only *the Martian spectrum.* Our Moon has no appreciable atmosphere. Consequently, its spectrum is the combined solar and terrestrial spectrum. If we compare the spectra of Mars and the Moon when these bodies are at the same altitude above our horizon,—that is when their light traverses the same thickness of terrestrial atmosphere,—and find that they differ in any respect, however slight, such difference must be caused by an atmosphere on Mars. If no difference is found to exist then the spectroscope affords no evidence of such an atmosphere. Thus the problem resolves itself into a comparison of the Martian and lunar spectra.

Thollon has found that in the combined solar and terrestrial spectrum three very strong groups of lines are produced by some of the *constant* elements of our atmosphere, probably by the oxygen. These are the Fraunhofer groups A, B and α, comprising about 130 separate lines. The presence of these lines indicates

the presence of *atmosphere*. If they are *stronger* in the Martian spectrum than in the lunar spectrum, that planet must have an atmosphere.

Thollon* found other groups of lines, comprising at least 1100 separate lines, produced by the *aqueous vapor* in our air. They have been divided by Thollon into the following seven groups:

(1)	Wave-lengths	745 to	716	(Fraunhofer's a)
(2)	" "	716 "	687	(below B)
(3)	" "	660 "	646	(around Hα)
(4)	" "	635 "	628	(near α)
(5)	" "	597 "	585	(around D)
(6)	" "	578 "	567	(Brewster's δ)
(7)	" "	548 "	542	

The presence of these groups of lines indicates the presence of *aqueous vapor*. If they are *stronger* in the Martian spectrum than in the lunar spectrum, there is *aqueous vapor* in the atmosphere of Mars.

Now while all these lines can be observed *individually* in the solar spectrum, owing to the high dispersion which can be used, they can only be observed as groups or bands in the Martian and lunar spectra, on account of the faintness of those spectra and the low dispersion which must be employed.

It is impracticable to observe the groups A, 745-716 and 716-687, which are at the extreme red end of the spectrum, and they will not be further considered. The atmospheric bands B and α are easy to observe in both spectra. The vapor groups of lines require great care in observing, for the reason that, owing to the low dispersion which must be used, the individual lines are not only blended with each other but also with the solar *metallic* lines which lie among them. In the 7th group, for instance, the vapor lines are so much fainter than the neighboring metallic lines that we need not consider that band in the present problem. For the same reasons the 6th group, 578-567, is not a sufficiently sensitive test for aqueous vapor, except in the Earth's atmosphere when the body observed is near the horizon. However, the region of the 6th group was carefully observed in the Martian and lunar spectra on several nights. The 4th group, 635-628, is useless as a test for aqueous vapor, since the faint lines composing it are always overwhelmed by the prominent lines in the atmospheric group α. Only the 3d and 5th groups remain available.

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* It must not be considered that the credit of this work is due wholly to Thollon. Many observers, Brewster, Gladstone, Janssen and others, investigated along the same lines. But Thollon's work is most complete and his maps are especially convenient and useful.

Of the 3d group I have not found useful that portion of it between 660 and 653, on account of the presence of the very heavy Hα solar lines and other solar lines among the relatively faint vapor lines. I have for my own use divided the rest of the 3d group into three parts, each of which was found useful. The first covers wave-lengths 6515-20 and includes about eight tolerably strong lines, the majority of which are vapor lines. Under all of the dispersions used it was simply a very narrow band or line, which I shall call c'. The second part covers the region 6491-6500. It includes half-a-dozen strong metallic lines and a few strong vapor lines, all, however, blending to form a very strong, narrow band or line, which we shall call c''. The third part is included between 6463 and 6490, which contains a great many vapor lines and a few metallic lines. It formed a band of good width which we shall call c'''.

The 5th group, extending from 597 to 585, I divided into four parts. The first covers wave-lengths 5941-5959; it contains a number of strong aqueous lines and several metallic lines, forming a band which I called d'. The second covers 5928-35; it is strong in neither metallic nor vapor lines. It forms a narrow band which I called d''. The third portion covers 5912-25; it contains a few metallic lines and very many strong vapor lines; I called this region d'''. The fourth covers 5884-5906; it contains the two very strong solar lines D_1 and D_2, several faint solar lines, and a great many vapor lines. It would be a very useful band if the D lines were not contained in it; but I found their presence very troublesome. Let us call this region d^{iv}.

For the reasons given above I confined my observations almost wholly to the groups B, α, c', c'', c''', d', d'', d''' and d^{iv}. Of these I found that α, c', c''', d', d''' were best suited for observation.

I observed the spectrum of Mars on ten nights between June 29 and August 10 of the present year, paying special attention to the nine critical groups of lines just mentioned. On eight of the nights I compared its spectrum with that of the Moon, when these two bodies were at equal distances above the horizon. On two nights, July 24 and 25, when the Moon was near the planet, I turned repeatedly from one spectrum to the other, while, on the former night, the planet passed from altitude 18° to 50°, and on the latter night, from altitude 45° to 55°. The two spectra have been compared when the relative humidity of our atmosphere was only 15 and when it was as high as 55. The observations were made principally with a dense 60° flint prism, with magnifying powers of 13 and 7, and occasionally with a 30°

prism and power 13. When the lunar spectrum was examined, the slit of the spectroscope was always shortened so that the lunar spectrum was of the same width as the Martian spectrum. The slit was directed always upon the brightest region of the Moon in order that the two spectra should be nearly of the same brightness, which is a very important condition in making reliable comparisons. In a word, the spectra have been compared under a variety of conditions, but with the conditions for the two bodies always identical. The atmospheric and aqueous vapor lines have been seen in both Mars and the Moon decreasing in intensity as these objects got higher and higher in the sky, and the aqueous vapor lines varying in intensity with the amount of moisture in our atmosphere. At all times the spectrum of Mars has appeared to be identical with that of the Moon in every respect.

Further, on several occasions when the planet's altitude was large, I examined the critical groups of lines, especially α, to determine whether the ends of the lines, which correspond to the limb of the planet, were stronger than their centers, which correspond to the center of the disk. The lines appeared to be of uniform intensity throughout, so far as the different intensities of different portions of the surface permitted a safe estimate to be made.

The intensity of the critical bands, α, for instance, was appreciably greater when the Moon and Mars were only 30° above the horizon than when they were 55°. The relative thicknesses of our atmosphere traversed by the rays when the bodies were at altitudes of 30° and 55° were as 2 to 1.22. If the rays of light from one of the bodies, Mars for instance, pass through a unit thickness of our atmosphere, and the rays from the Moon pass through 1½ units, the intensity of α in the spectrum of the latter is certainly greater than in the spectrum of the former. In fact, I am quite confident that a difference of 25 per cent in the lengths of paths traversed by the rays from the two bodies would cause an appreciable difference in the intensities of their α bands. The accuracy of the observation is greatly increased by the presence of several neighboring metallic lines which can be used as standards of comparison.

The results of these observations are as follows:

First.—The spectra of Mars and our Moon, observed under favorable and identical circumstances, seem to be identical in every respect. The *atmospheric* and *aqueous vapor* bands which were observed in both spectra appear to be produced wholly by

the elements of the Earth's atmosphere. The observations, therefore, furnish no evidence whatever of a Martian atmosphere containing aqueous vapor.

Second.—The observations do not prove that Mars has *no* atmosphere similar to our own; but they set a superior limit to the *extent* of such an atmosphere. Sunlight coming to us *via* Mars would pass twice either partially or completely through his atmosphere. If an increase of 25 to 50 per cent in the thickness of our own atmosphere produces an appreciable effect, a possible Martian atmosphere one-fourth as extensive as our own ought to be detected by the method employed.

Third.—If Mars has an atmosphere of appreciable extent, its absorptive effect should be noticeable *especially* at the limb of the planet. My observations do not show an increased absorption at the limb. This portion of the investigation greatly strengthens the view that Mars does not have an *extensive* atmosphere.

While I believe that the polar caps on Mars are conclusive evidence of an atmosphere and aqueous vapor, I do not consider that they exist in sufficient quantity to be detected by the spectroscope. This view has an important bearing upon the questions relating to the low albedo of the planet, and the well-known brightness of its limb, in both of which respects the planet resembles our Moon.

MOUNT HAMILTON, 1894, August 14.

ON THE LINE SPECTRUM OF OXYGEN.*

B. HASSELBERG.

Under the above title Herr Eisig has recently published in the *Annalen* the results of his researches on the subject to which reference is made. The object of the researches was to photograph all the lines of pure oxygen, and to determine their wave-lengths with the greatest possible precision, without reference to earlier results. To attain this end, the rarified gas was enclosed in Geissler tubes, its spectrum was photographed on plates 50 cm. long with the aid of the large Rowland concave grating of Messrs. Kayser and Runge, and by measurement with a dividing engine the lines were referred to the lines of an iron spectrum photographed on the same plate. The wave-lengths obtained were

* Translated from the *Annalen der Physik und Chemie*, No. 8, 1894. An abstract of the observations reviewed by Professor Hasselberg is given in ASTRONOMY AND ASTRO-PHYSICS, June, 1894, p. 505.

therefore according to the system of Rowland, which Kayser and Runge made the basis of their determination of wave-lengths in the spectrum of iron. Considering the great sharpness which is afforded by the photographic method, particularly with the aid of Rowland's gratings, one would have good reason to expect that the wave-lengths of the lines of the gas would be obtained with an exactness nearly equal to that of the metallic lines phographed with the same apparatus. This, however, seems to be by no means the case; for the author himself states that, although the measurements were made to hundredths of a tenth-meter, the results for different plates sometimes varied by as much as 0.3 tenth-meter. As the author points out, the cause of this is to be sought in displacements, or changes of temperature of the apparatus during the long exposure of from two to four hours; and one can only agree with him when he rounds off the definitive means to the nearest tenth of a tenth-meter, and estimates the possible uncertainty at from one to two tenths.

Under these circumstances the question may well be raised, whether our knowledge of this spectrum has really been appreciably widened by the investigation referred to. I believe that it has not. It is true that the determinations of the author, as compared with those of Schuster, Deslandres, Hartley and Adeney, must be allowed to have a certain superiority with respect to completeness, and perhaps also to accuracy, but they are hardly superior to those of Trowbridge and Hutchins, which were carried out with the same means, but according to a method less liable to systematic errors. The researches last mentioned, although not entirely free from objection in certain details, are in the main to be regarded as quite satisfactory.

Besides the investigations mentioned above, are some others,* unknown to the author, which were made in the physical institute of this place in 1891, and in which, with far simpler means, a degree of accuracy was reached at least equal to that of the results under consideration. This investigation, the principal object of which was to separate the lines of nitrogen as completely as possible from those of oxygen in the spectrum of air, was carried out with the aid of a spectrograph constructed of two large Steinheil prisms of flint glass and a pair of telescopes, of which the one serving as camera had a focal length of four feet. With the aid of this apparatus the spectrum was photographed with that of the Sun on plates whose dimensions were 7.5 by 3.5 centimeters. The wave-lengths were determined without micro-

* Neovius, *Bihang til K. Svenska Vet. Akad. Handlingar. Bd. 17, Afd. I, Nr. 8.*

metric measurement, by comparison with Rowland's map. Comparing the results of Neovius with those of the author, we have the following table:

Eisig.		Neovius.		Eisig.		Neovius.		Eisig.		Neovius.	
λ	I	λ	I	λ	I	λ	I	λ	I	λ	I
3712.8	4	3712.9	3	4111.2	6	4111.0	3	4369.7	6	4369.7	4.3
27.5	3	27.4	2	12.4	5	12.2	3	96.4	5	96.3	4.3
49.6	2	49.8	1	14.2	6	14.0	4	4115.3	1	15.0	1
54.7	6	54.7	4	19.5	2	19.3	2	17.4	2	17.2	1
57.3	6	57.1	5	20.5	4	20.4	3	43.6	6	43.0	4.3
60.0	6	59.9	4	21.7	6	21.6	3	48.7	6	48.2	4.3
3851.2	6	51.3	3	33.2	5	33.0	2	52.8	5	52.4	4.3
57.4	6	57.5	4	42.4	6	42.3	3.4	65.8	6	65.3	3.4
64.8	5	64.9	3.2	44.0	6	43.6	3.4	66.7	6	—	—
82.5	3	82.7	2.3	46.3	5	46.1	3	68.4	6	67.8	4
3907.6	6	07.7	4.3	53.7	3	53.6	1	69.9	6	69.4	4.3
12.3	3	12.3	2.1	56.8	6	56.7	3	4591.4	3	91.0	2
19.6	4	19.5	3.2	69.5	6	69.5	3.2	95.5	4	96.3	3.4
45.3	4	45.3	3.2	85.8	4	85.5	1	4639.2	4	38.9	3.4
54.5	3	54.7	1.2	90.0	4	90.0	1	42.1	2	41.9	3.2
73.4	1	73.5	1	4317.4	3	17.1	2.1	49.5	1	49.2	2.3
83.0	4	83.0	3.2	19.9	3	19.6	2.1	51.2	4	51.0	4.3
4070.1	1	70.1	2	26.2	6	25.8	3.4	62.0	4	61.6	3.2
72.5	1	72.4	2	27.8	6	27.5	4.3	74.2	6	73.5	5
76.2	1	76.2	1	29.0	6	28.4	4	76.6	5	76.6	3
79.1	5	79.0	3.4	32.2	6	32.0	5	96.8	6	96.0	4.5
85.5	4	85.2	3	37.3	4	37.0	4.3	99.6	4	99.4	3
93.2	5	93.0	3	45.9	2	45.4	3.2	4701.5	6	—	—
96.9	6	96.9	4	47.8	3	47.5	4.5	03.4	6	—	—
97.8	4	—	—	49.8	1	49.3	2	05.7	4	05.6	3
4103.4	6	03.1	4.3	51.7	2	51.4	3	10.4	5	10.1	3
05.3	4	05.1	1.2	67.3	3	67.0	2.3				

The wave-lengths given above are the means of two series of determinations, made respectively with copper and aluminium electrodes. In general, the probable error of Neovius' results may be estimated at about ± 0.1 tenth-meter, and the accuracy may be regarded as the same in both cases.

If now the two columns are compared, it will be seen that the values of Eisig are almost invariably a little greater than those of Neovius,—on the average by about 0.2 tenth-meter,—and also that the differences increase with the wave-length. Leaving out of consideration this systematic difference, which under the circumstances is of comparatively small importance, we may regard the agreement as quite satisfactory, and consider the two columns to be of equal weight. The series of Neovius is however, really somewhat the more complete, provided that several weak lines, not given in the table, belong without doubt to oxygen.

From the above comparison it seems to me that the work of the author has not added much to our knowledge of the subject, unless it be the experience that for the investigation of faint spectra, such as those of the gases, the large grating spectroscope has no appreciable advantage over simpler apparatus.

ON THE ASTIGMATISM OF ROWLAND'S CONCAVE GRATINGS.

DR. J. L. SIRKS, GRONINGEN.

In a well-known paper of Mr. J. S. Ames, On Concave Gratings for Optical Purposes*, the following passage occurs. "Owing to "the astigmatism of the grating, it is not possible to adopt the "usual method of illuminating part of the slit with the solar im-"age and part with the spark or arc; and so a diffierent and far "better plan is adopted. A compound photograph of the two "spectra is taken in the following manner."

Yet this new plan, devised and executed by Professor Rowland with his wonted success, is only applicable by means of photography, as the photographs of the different spectra must be taken one after another; and if the precited statement,—which, so far as I see, neither Mr. Ames nor Professor Rowland, at whose request he wrote, has recalled or modified—were to be accepted in its apparent purport, the beautiful instrument with which Mr. Rowland has endowed the spectroscopist would be unfit for the direct comparison of spectra from different sources by ocular observation, that was always regarded as a precious function of the dioptric spectroscope.

Fortunately however, though in the literal acceptation of the words it is useless to illuminate *part* of the slit with one source of light and *part* with another, it is certainly possible to institute the intended comparison, at least with the first and second spectra, by a slight modification of the common method: the prisms or other equivalent contrivances that are generally used to introduce lateral beams of light, need only be placed *not against the slit*, in A (Fig. 1), *but at a distance* $\rho \sec \nu - \rho \cos \nu$ *from the slit*, $\rho \sec \nu$ from the grating, viz., at a point Q, being the intersection of BA and the tangent in the focus C.

In order to demonstrate the truth of this assertion let us consider the pencil of monochromatic rays that will, after the reflec-

* *Phil. Mag.* XXVII, p. 381, 1889; cf. ASTR. AND ASTRO-PH. 1892, p. 39. *Verhand Kon. Akad. v. Wetensch.* (*le Sectie*). Dl. II.

tion at the grating, concur in the focus at C. It may be divided into what I may be allowed to call vertical "fans" of rays, each of them being limited by two vertical planes passing through the slit and including an infinitesimally narrow strip of the grating. Now all the rays contained in such a "fan", in order to concur at C without any difference of path, must issue from an apex situated in the line CQ, being the axis of the spherical surface, part of whose equatorial region is occupied by the grating.

On the other hand the horizontal fans of rays into which the pencil may be divided, by theory of diffraction have their apices in the slit. So *all* the rays that concur at C must have passed successively through two caustics: the one realized by the slit, the other only virtual, lying along the line GH, where it may be realized by another slit, if the source of light be placed at a sufficient distance. It will be easily seen that the length of the first caustic, the available part of the slit, is $b \times QA / QB = b \sin^2 \nu$, that of the second $GH = a \times QA / BA = a \operatorname{tg}^2 \nu$, a and b being the horizontal and vertical dimensions of the grating.

The existence of the second caustic, that is of great importance for the complete theory of the instrument, may be very simply demonstrated *ad oculos* by stretching a thin wire in Q along GH across an incident beam of sunlight: the result is a *perfectly defined* narrow black band passing horizontally across the field of the eyepiece. Any other horizontal strip of the field has its own conjugate horizontal strip, of a somewhat greater width, in the proportion $\rho \sec \nu / \rho$, a little above or below Q in a vertical plane passing through GH. Yet every single *point* in the strip of the field, belonging to one single λ, derives from the conjugate horizontal caustic // GH in its full *length*; conversely every *point* of a horizontal slit above or below GH has its horizontal *linear* image in the field depicted by rays of different λ's.

If the horizontal band, seen in the field, is required to have a width h, the horizontal slit in GH must be replaced by a rectangular diaphragm height $h \times QB / BC = h \sec \nu$, length as before $GH = a \operatorname{tg}^2 \nu$. At the same time the vertical slit ought to be lengthened by the quantity $h \cos \nu$, until it gives passage to all the rays issuing from the diaphragm that can reach the grating; so the full length becomes $h \cos \nu + b \sin^2 \nu$. All the rays that are obstructed by the diaphragm, if admitted would only tend to increase the disadvantageous illumination of the field by scattered light.

Any incident ray passing through the diaphragm *over* (under) the line CQ and through the slit will come at a focus in the *lower*

Fig. 1.

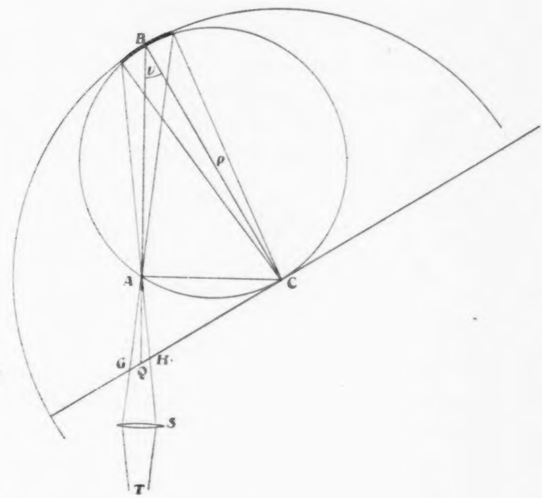
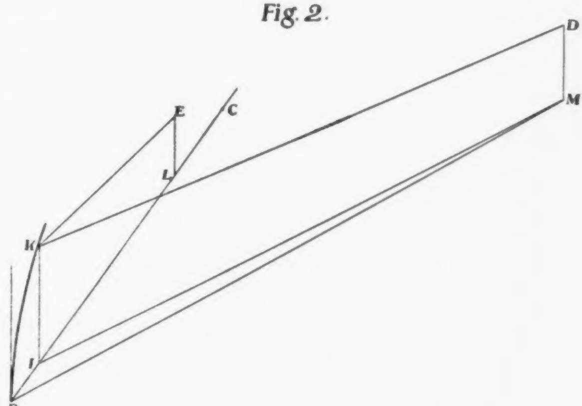


Fig. 2.



J. L. Sirks' Astigmatism of Rowland's Concave Gratings.

(upper) half of the field. A short but rather broad prism, 2 or 3 mm. in height, placed at Q and reflecting lateral solar light, will give a narrow solar spectrum with perfectly defined edges, passing through the center of the field; at the same time it will obstruct *none* of such rays, emanating from a sodium-flame or arc-light placed somewhere about T, as may concur in forming a sodium- or metal-spectrum in the remaining part of the field. Of course if we wish to get the metal-spectrum as bright as possible, the cone of light furnished by the condensing lens S must be wide enough to fill up the wedge formed by the rectangular diaphragm and the slit.

With the third and ulterior spectra and with a very large grating the condensing lens should be of rather great dimensions, so I think the method will only be quite applicable with the first and second spectra. I may add that probably the very best plan would be to have a bicylindrical lens, or two cylindrical lenses put crosswise, of such a curvature that both its orthogonal caustics might coincide with the above named caustics of the grating; but every different angle ν or at least every successive spectrum would require its especial lens.

Through the kind permission and efficacious assistance of Professor Haga I have been able to control the above by a provisional experiment. A narrow central band of the field on a black ground showed the first sodium-spectrum originating from a strip of mirror-glass, height 2.5 mm., placed along the caustic at Q, at 171 mm. from the slit, and upon which the light of a lateral Bunsen-flame was concentrated through a lens, $f = 150$ mm. The strip of glass just arrested the superfluous central part of a direct beam of sunlight that filled out the upper and lower parts of the field with its spectrum. The sunlight had to be passed through several layers of wire-gauze in order to bring down its intensity to that of the reflected sodium light. Now in the compound spectrum the two positive sodium-lines ended abruptly where the negative sodium-lines began; yet two very narrow sharp black lines, about 0.1 mm. wide, separated the three contiguous spectral bands: this was occasioned by the strip of glass having been simply cut with a diamond without any ulterior grinding or polishing; so the somewhat rugged edges, while they were unable to take part in the reflection of the sodium-flame only acted as a barrier against the sunlight grazing them.

In order to try to what limit, if need be, the method can be applied, we turned the moveable girder of the spectroscope on to the last or fourth spectrum with $\nu = 68°$, sin $\nu = 0.928$. A knit-

ting needle held in the horizontal caustic, that now lay at 714 cm. from the slit, was accurately represented by a narrow black line across the solar spectrum. This proves that the definition in the images of horizontal lines, produced by the vertical fans holds good even at this great angle of incidence.

I still may remark that the whole action of the hollow grating, with a radius ρ, may *for these fans* be regarded as the result of three successive operations: one being that of a first concave mirror, with a radius 2ρ, but reduced by astigmatism to a radius $2\rho \sec \nu$, that brings the incident rays to parallelism; the second that of a plane grating, which occasions the diffraction at an angle ν; the third that of another concave mirror 2ρ, which makes the diffracted parallel rays converge into a focus. The distances and dimensions of two conjugate images may be simply calculated by the formulæ for one mirror with $f = \rho/(1 + \cos \nu)$, as may be proved in the following manner.

Let BK (Fig. 2) be part of a very narrow vertical strip, and B the center of the mirror, C the center of curvature; D and E two conjugate foci determined by their height $z_1 = \mathrm{DM}$, $z_2 = \mathrm{EL}$ over the horizontal plane LBM, by $\mathrm{BM} = \mathrm{R}$, $\mathrm{BL} = r$ and angle $\mathrm{MBL} = \nu$; ρ being the radius BC of the sphere, $\mathrm{KI} = l$.

Now with a sufficient degree of approximation we successively find

$$\mathrm{BI} = \frac{l^2}{2\rho}, \quad \mathrm{IM} = \mathrm{R} - \frac{l^2 \cos \nu}{2\rho},$$

$$\mathrm{KD}^2 = \mathrm{IM}^2 + (l - z_1)^2 = \mathrm{R}^2 - \frac{\mathrm{R}l^2 \cos \nu}{\rho} + (l - z_1)^2$$

$$\mathrm{KD} = \mathrm{R} - \frac{l^2 \cos \nu}{2\rho} + \frac{l^2 - 2lz_1 + z_1^2}{2\mathrm{R}};$$

consequently

$$\mathrm{KE} = r - \frac{l^2}{2\rho} + \frac{l^2 - 2lz_2 + z_2^2}{2r}.$$

For the point B, $l = 0$, we have

$$-\mathrm{BD} = -\mathrm{R} \qquad -\frac{z_1^2}{2\mathrm{R}},$$

$$-\mathrm{BE} = -r \qquad -\frac{z_2^2}{2r}.$$

Hence by addition we find for the difference $\varDelta$ of the two paths DKE — DBE

$$\varDelta = l^2\left(-\frac{1 + \cos \nu}{2\rho} + \frac{1}{2\mathrm{R}} + \frac{1}{2r}\right) - l\left(\frac{z_1}{\mathrm{R}} + \frac{z_2}{r}\right).$$

Now if indeed D and E be conjugate foci, $\varDelta$ must vanish for every value of l, and both the factors included in brackets must be $= 0$. So the first factor gives for the relation of the distances

$$\frac{1}{R} + \frac{1}{r} = \frac{1 + \cos \nu}{\rho}$$

as with a mirror of ρ $(1 + \cos \nu)$ focus; the second makes

$$\frac{z_1}{z_2} = -\frac{R}{r}$$

so that the heights of the images are proportional to the distances as in common optics.

I think I have shown that the astigmatism of the grating, while securing to the instrument some precious qualities, is no impediment against a method of observation that seems to be reputed incompatible with astigmatism. On the other hand the valued quality of the concave grating, that it shows no dust-lines, and that the image of a star or a spark on the slit is broadened out into a band, may be imparted to a dioptic spectroscope by giving a slight convex spherical curvature to one side of one of the prisms, so that the instrument becomes slightly astigmatic.

Dec. 28th, 1893.

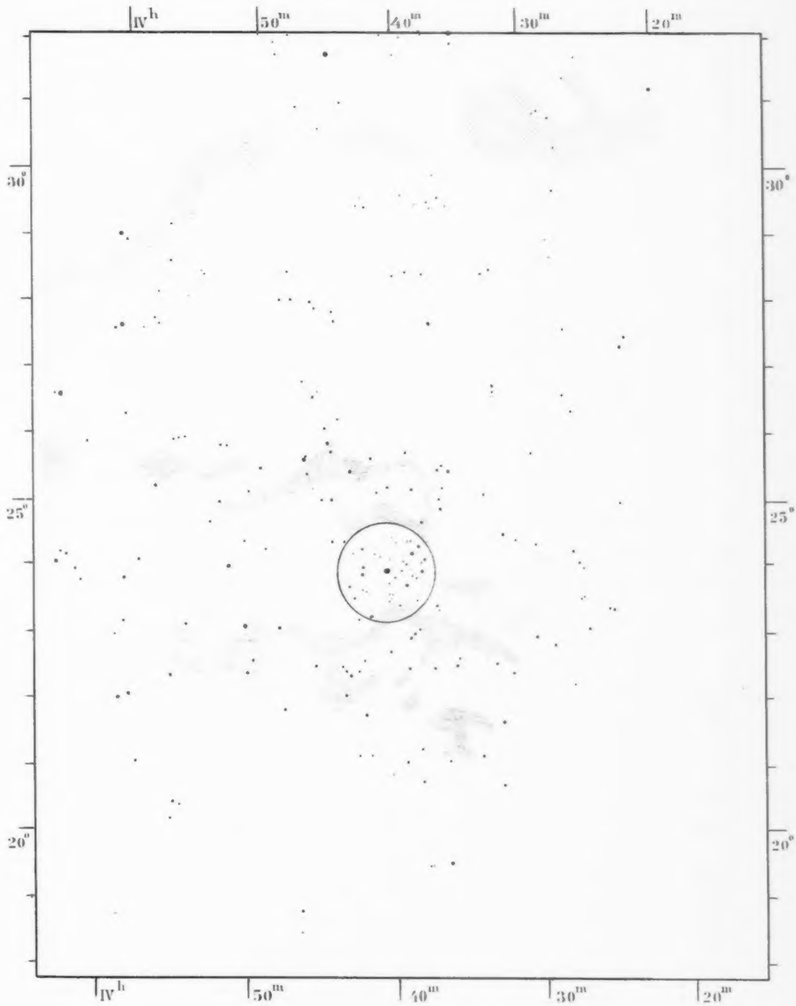
ON THE EXTERIOR NEBULOSITIES OE THE PLEIADES.

E. E. BARNARD.

For many years during my comet seeking I have known of a vast and extensive but very diffused nebulosity north of the Pleiades. Other masses of this diffused matter make their presence known by a general dulling of the field when sweeping in the region of the cluster.

As is well known the immediate group of the Pleiades is filled with nebulosity which in general attaches itself to the various stars and is of a wispy and streaky nature. This is well shown on the photographs made by the Henry Brothers, by Roberts, Wilson and others. In these pictures, however, the nebulosity seems limited to a small area included in the cluster itself. The principal and best known of these nebulosities is the one discovered by Tempel in 1859 and which envelopes Merope and extends southwesterly from that star. The other nebulosities have revealed themselves through the aid of photography in the past ten years.

PLATE XXXIII.



DISTANT NEBULOSITIES AROUND THE PLEIADES.

This cluster and its nebulosities have been more thoroughly studied visually and photographically than any other group of stars in the sky. Consequently anything new about it is of the highest interest.

It has been my hope during the past two or three years to some time be able to secure a photographic impression of these vague nebulosities that I had seen in the telescope. It was evident this would require a long exposure. The mounting of our Willard lens does not permit an exposure to be carried beyond the meridian. To get sufficient time would therefore require more than one night.

This past winter I have been able by carefully inclosing the camera box in thick black cloth and by taking other precautions, to extend the exposure through two nights with success.

Previous to this I gave an exposure on the Pleiades of four hours, which showed all the well known nebulosities, and gave faint suggestions of more distant wisps of nebulæ.

December 6, 1893, an exposure was begun which was continued for five hours. The lens was then carefully covered without disturbing the plate. The next night was cloudy but on December 8 the exposure was continued for five hours and fifteen minutes—making thus a total of $10^h\ 15^m$.

The resulting picture confirmed the first photograph and showed a number of singular curved and streaky nebulosities apparently connected with the Pleiades and extending all about the group.

Some of these streams extend irregularly for several degrees each side of the cluster—especially towards the east.

To give an idea of the affected regions, I have made the inclosed drawing from the photograph which will explain itself. On this I have drawn a circle about the Pleiades inside of which all the nebulosity shown in previous photographs has been confined.

I have not attempted to sketch the nebulosities connected immediately with the stars of the cluster, and shown on the photograph, for these are already well known to everybody.

For the more ready location of these outer nebulosities I have, very roughly, put a set of coördinates around the edge of the drawing.

To the north of the Pleiades, from $\alpha = 3^h\ 20^m$ to 4 hours and beyond, and from $\delta = +30°$ to several degrees further north, is a region singularly devoid of small stars but covered with large masses of very diffuse nebulosity; this part of the sky will attract the attention of any one in sweeping over it with a very low

power on an ordinary telescope. The field is dull with feeble nebulosity. This region is partly shown on the northern part of the plate and the nebulosity is evident. This is about and west of the stars ζ and o Persei.

During the coming winter I hope to be able to further extend the exposure time for the delineation of other and fainter nebulosities, in the region of the Pleiades, that are too vaguely shown on the present plate to make much out of.—*Astronomische Nachrichten*, No. 3253.

MOUNT HAMILTON, 1894, July 25.

ASTRO-PHYSICAL NOTES.

All articles and correspondence relating to spectroscopy and other subjects properly included in ASTRO-PHYSICS, should be addressed to George E. Hale, Kenwood Observatory of the University of Chicago, Chicago, U. S. A. Authors of papers are requested to refer to last page for information in regard to illustrations, reprint copies, etc.

A Large Telescope for the Cape Observatory.—We learn from the October *Observatory* that Mr. Frank McClean has offered to present to the Royal Observatory of the Cape of Good Hope a large equatorial for photographic and spectroscopic work. The photographic objective will have an aperture of 24 inches, and an objective prism of 7½° refracting angle and 24 inches aperture is to be provided. The visual telescope which is to be mounted with the photographic instrument, will be of 18 inches aperture. A slit spectroscope is also to be provided, for work on the radial motion of stars. The construction of the instrument is already well under way in the workshops of Sir Howard Grubb.

The importance of this splendid gift is greatly increased by the fact that it is intended for use in the southern hemisphere, where the opportunities for astro-physical research are practically unlimited. Of Mr. McClean's excellent spectroscopic work at Tunbridge Wells we have frequently had occasion to speak in this journal. He is to be congratulated for his broad-minded generosity and scientific discrimination in making this munificent gift.

Professor Hartley's Observation of the C Line of Hydrogen in the Bessemer Flame.—Many attempts have been made to produce the spectrum of hydrogen by means other than electrical, but if a single observation by Watts of the C line in a Bessemer flame be excepted, no certain proof is offered that success has been attained. Professor W. N. Hartley has been engaged for some time upon an exhaustive study of flame spectra at high temperatures, and several papers dealing with these investigations have appeared in the *Philosophical Transactions* and the *Proceedings* of the Royal Society. The last number of the *Proceedings* (Vol. LVI, No. 337) contains an interesting paper on the spectrum of the Bessemer flame. About ninety spectra were photographed; the region studied extended from λ 7697 to about λ 3380.8. The least refrangible line photographed was that of lithium at λ 6707. About ninety-two lines were identified with lines in the solar spectrum, with lines in Kayser and Runge's photographic map of the

iron spectrum, and with lines in the oxyhydrogen flame spectra of steel and ferric oxide. In the flame issuing from the mouth of the converter during the first period of the "blow" the lines of the alkali metals sodium, potassium and lithium were seen unreversed on a bright continuous spectrum caused by carbon monoxide. "The C line of hydrogen and apparently the F line were seen reversed during a snow storm." No special attention appears to have been given to this observation, but there seems to be no doubt in the mind of the author that the C line was actually seen. The paper is unfortunately an abstract, and the dispersion employed is not stated, though the contest seems to indicate that it was high enough to leave little doubt, if the identification was carefully made. It thus appears that Watts' observation, which was made in rainy weather, is confirmed. If this is the case a result of some importance in astro-physics has been obtained. It was once supposed that the H and K lines of calcium could be produced only by electrical means, but they were finally discovered in the flame of burning magnesium ribbon which contained calcium as an impurity. In the case of hydrogen it has been argued that solar prominences are electrical phenomena, because they show the lines of this gas, which are obtained in the laboratory only by electrical means. If Professor Hartley's observation is substantiated this argument must fall to the ground. Whether the chemical action involved in the present case is necessary to the production of the hydrogen lines is a separate question, about which there will be much difference of opinion.

Note on the Spectrum of Mars.—Professor Campbell in his paper on the Spectrum of Mars (Publ. A. S. of the Pacific, Vol. VI p. 228)* in speaking of the early observations of myself and others, says:—

"It is very important, as Janssen pointed out in 1867, that we eliminate as "far as possible the effect of aqueous vapour in our own atmosphere. The observers "do not seem to have taken this factor into account (except Janssen, the details "of whose observations appear to be unpublished.)"

So far is this statement from being correct that the method of eliminating the effect of our atmosphere by observations of the Moon at the same time was that originally employed by me in 1867. In speaking of faint lines seen on both sides of D, and which appeared to indicate terrestrial gases or vapours in the atmosphere of Mars, I say expressly:—

"That these lines were not produced by the portion of the Earth's atmosphere "through which the light of Mars had passed, was shown by the absence of simi- "lar lines in the spectrum of the Moon which at the time of observation had a "smaller altitude than Mars."—(*M. N. Roy. Ast. So.*, Vol. XXVII p. 178.)

In 1879 I took photographs of the spectra of Mars and some other planets in the twilight, simultaneously with spectra of the light from the sky immediately about the planets. In these spectra extending from *b* to S in the ultra-violet, no lines, or modifications of the spectrum appear, which are peculiar to the planet's spectrum.

The apparatus necessarily employed by me at the early date of 1862-7 was very imperfect as compared with the instruments now in use, but I have no reason to doubt the substantial accuracy of the observations which were made with much care. WILLIAM HUGGINS.

Upper Tulse Hill, October 6th, 1894.

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* See page——

The Advantage of the Short-Focus Camera in Spectrum Photography.—The notes by Dr. Huggins and Professor Campbell (ASTRONOMY AND ASTRO-PHYSICS, No. 127, p. 568, and No. 128, p. 696), on the advantage of short-focus camera in photographic work with the astronomical spectroscope, seem to leave some points not quite clear, and a few further comments on this subject may not be out of place. The advantage of the short camera in ordinary spectrum photography is well known (it is stated for instance on page 374 of the *Encyclopedia Brittannica*), and it applies equally to the spectroscope used in connection with a telescope when the source of light is an object of considerable angular magnitude. I do not see that the conditions on which it is based hold when the source of light is a star. The photographed spectrum must have a certain breadth, which is determined by the following considerations: it must subtend a sufficient angle when seen under the measuring microscope, which usually has a power of from ten to thirty diameters, and it must be so large that accidental combinations of silver grains, specks of dust, or blemishes on the plate may not be mistaken for lines. This minimum breadth is, according to my experience, about one quarter or one-third of a millimetre, and the experience of the Potsdam observers leads them to the same conclusion. As the breadth of the spectrum is obtained by allowing the star to travel along the slit, it is evident that if the camera is shortened the amount of drift given to the star must be proportionally increased, and hence the exposure must be longer. The same law does not hold, therefore, as in the case of a large object, when drift becomes unnecessary.

When the image on the slit-plate is kept stationary, its size determines the breadth of the spectrum. Hence the short camera might be advantageously used with a large telesocpe, and not perhaps, with a small one on the same object. It may also happen, in cases where exact measurement of the photographs is required, that the greater narrowness of the lines on the smaller image is not a sufficient compensation for the reduction of the scale. We may say, therefore, it seems to me, that the short camera may be used to advantage when the size of the image on the slit-plate, as determined by the size of the telescope and the angular magnitude of the object, or the general purpose of the investigation, allows us to sacrifice the scale of the photographs for the sake of obtaining intensity.

Some of these points were touched upon by Professor Campbell in his first paper on the spectrum of the Orion Nebula (ASTRONOMY AND ASTRO-PHYSICS, May, 1894). I desire more particularly to call attention to the facts that the case of a star is different from that of other objects, and that a large telescope has a great advantage over a small one in the number of cases to which the short camera is applicable.

J. E. KEELER.

The Dispersion of Fluorite and of Rock-Salt.—In connection with his researches on the radiation of heat by gases, Dr. Paschen has made a study of the dispersion of the two substances mentioned above. The most remarkable result is that beyond a certain wave-length the dispersion begins to increase; *i. e.*, the curve $n = f(\lambda)$ is concave toward the axis of λ. The observations in the lower spectrum are not satisfied by Briot's formula with constants derived from observations in the upper spectrum, and Dr. Paschen gives a mathematical proof of the impossibility that they should be. It follows that all previous determinations of wave-lengths beyond 5μ are too great, as they are based on exterpolations which assume that the curve beyond this point is either straight or slightly convex. Dr. Paschen considers it doubtful whether any heating effect has ever been observed in the spectrum beyond 15μ.

A consideration of the relative dispersion and absorptive action of fluorite and rock-salt in different parts of the spectrum shows that a prism of fluorite is the more advantageous for wave-lengths above 8μ; for longer wave-lengths rock-salt should be used.

PLANET NOTES FOR DECEMBER.

H. C. WILSON.

Mercury will be morning planet during December and may be seen toward the southeast between six and seven o'clock during the first half of the month. The best observations will be obtained, however, about ten or eleven o'clock when the planet is near the meridian.

We would remind those of our readers, who receive this number of ASTRONOMY AND ASTRO-PHYSICS before Nov. 10, of the transit of Mercury which is to occur on that date. The transit will begin at $9^h\ 55^m$ A. M. and end at $3^h\ 12^m$ P. M. Central time. For other data see last number.

Venus will be evening planet but will set too soon after the Sun to be seen.

Mars will be in excellent position for evening observation. He will be near the meridian between seven and eight, at a good altitude so that, although this is a cold month, some good views ought to be obtained. Mars will be in conjunction with the Moon, about 2° south, Dec. 8 at noon.

The seeing at Northfield has not been of the best this year and we have but few really good views of Mars. Our best view was the night of Oct. 8, when the gulf Auroræ Sinus, longitude 55° was on the Martian meridian, (see Schiaparelli's map reproduced in our last number). The appearance of the planet was quite different in many respects from that indicated by the map. Solis Lacus was not round or oval, but was broader at the east end, agreeing almost exactly with the map drawn by Proctor years ago. The "canals" Ambrosia, Nectar, Tithonius, Fortuna, Chrysorrhoas and Ganges were seen, some of them in slightly different positions from those shown in the map. Fons Juventæ was also seen and a canal, not on the map, running northward from it.

Mare Erythraeum with the light regions Deucalionis Pyrrhæ and Protei appeared much as shown in the map but Mare Australe did not show dark at all. All the south polar region except the snow-cap itself appeared of the same yellowish hue with the north equatorial regions.

The south polar cap was quite small and on Oct. 10 and 12 was still smaller, measuring only a small fraction of a second of arc. On the 16th it had entirely disappeared. Since that time to the present writing (Oct. 23) the snow-cap has been entirely invisible. We do not remember that such a total dissappearance has ever been recorded before, although the great diminution of the polar cap in the summer season has been a well known fact.

Jupiter may be observed during the whole night in December. He will be in conjunction with the Moon, 5° to the south of the latter on the morning of the 13th, and at opposition to the Sun on the 22d. The nights will be cold but a good view of Jupiter's belts well repays the observer for the little suffering that must be endured to get it.

Saturn and *Uranus* are morning planets and will probably not be observed much by the amateur in the cold weather. Saturn is at the feet of Virgo moving slowly eastward. Uranus is in Libra a little south of the star ν.

Neptune may be observed all night. He is in Taurus a little south of the star ι, and moving very slowly westward. Neptune will be at opposition Dec. 6.

Planet Tables for December.

[The times given are local time for Northfield. To obtain Standard Times for Places in approximately the same latitude, add the difference between Standard and Local Time if west of the Standard Meridian or subtract if east].

Date.	R. A. h m	Decl. ° ′	Rises. h m	Transits. h m	Sets. h m
		MERCURY.			
Dec. 5	15 33.4	− 17 31	5 44 A. M.	10 35.6 A. M.	3 27 P. M.
15	16 33.1	− 21 33	6 23 "	10 55.6 "	3 28 "
25	17 38.7	− 24 08	7 02 "	11 21.9 "	3 42 "
		VENUS.			
Dec. 5	16 54.3	− 22 37	7 34 A. M.	11 56.3 A. M.	4 19 P. M.
15	17 49.0	− 23 51	7 55 "	12 11.4 P. M.	4 25 "
25	18 43.0	− 23 51	8 11 "	12 27.0 "	4 43 "
		MARS.			
Dec. 5	1 23.3	+ 9 10	1 44 P. M.	8 23.9 P. M.	3 04 A. M.
15	1 31.4	+ 10 18	1 08 "	7 52.6 "	2 37 "
25	1 42.9	+ 11 40	12 34 "	7 24.7 "	2 15 "
		JUPITER.			
Dec. 5	6 15.0	+ 23 09	5 32 P. M.	1 14.8 A. M.	8 58 A. M.
15	6 09.5	+ 23 12	4 46 "	12 29.9 "	8 13 "
25	6 03.6	+ 23 14	4 01 "	11 44.7 P. M.	7 28 "
		SATURN.			
Dec. 5	14 06.8	− 10 24	3 47 A. M.	9 9.6 A. M.	2 32 P. M.
15	14 10.6	− 10 42	3 13 "	8 34.0 "	1 55 "
25	14 14.0	− 10 58	2 39 "	7 58.2 "	1 18 "
		URANUS.			
Dec. 5	14 59.9	− 16 41	5 07 A. M.	10 02.1 A. M.	2 56 P. M.
15	15 02.2	− 16 51	4 31 "	9 25.0 "	2 20 "
25	15 04.2	− 17 00	3 54 "	8 47.7 "	1 42 "
		NEPTUNE.			
Dec. 5	4 53.1	+ 21 01	4 20 P. M.	11 52.9 P. M.	7 25 A. M.
15	4 51.9	+ 21 00	3 40 "	11 12.5 "	6 45 "
25	4 50.7	+ 20 58	3 00 "	10 32.0 "	6 04 "
		THE SUN.			
Dec. 5	16 49.0	− 22 27	7 21 A. M.	11 51.0 A. M.	4 21 P. M.
15	17 33.0	− 23 19	7 30 "	11 55.5 "	4 21 "
25	18 17.4	− 23 23	7 36 "	12 00.5 P. M.	4 24 "

Occultations Visible at Washington.

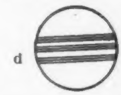
Date 1894	Star's Name.	Magnitude.	IMMERSION Washington M. T. h m	IMMERSION Angle f'm N p't. °	EMERSION Washington M. T. h m	EMERSION Angle f't N p't. °	Duration. h m
Dec. 5	B A.C. 8184	6	8 31	76	9 39	207	1 08
7	70 Piscium	8	11 07	100	12 00	198	0 53
9	27 Arietis	6	3 49	40	4 43	260	0 54
10	9 Tauri	7	8 51	126	9 27	181	0 36
10	*g* Pleiadum	6	13 38	33	14 27	304	0 48
10	17 Tauri	4	13 25	74	14 35	262	1 10
10	23 Tauri	5	14 15	128	14 59	212	0 44
10	24 Tauri	8	14 38	96	15 38	245	1 00
10	η Tauri	3	14 42	99	15 41	243	0 59
10	B.A.C. 1171	8	15 14	57	16 08	287	0 54
10	27 Tauri	4	15 32	128	16 13	217	0 41
10	28 Tauri	6	15 28	107	16 21	238	0 53
13	47 Geminorum	6	17 16	85	18 11	311	0 55
16	34 Leonis	6	15 42	114	16 57	319	1 15

Jupiter's Satellites for December.

Phases of the Eclipses of the Satellites for an Inverting Telescope.

I.

III.



II.

IV.

No Eclipse.

Configurations at 11h for an Inverting Telescope.

Day.	West.		East.
1	·3 2·	○	1· ·4
2	·2	○	·3 4· ·1●
3	1·	○	·2 .3 4·
4		○	·12· 3·4·
5	2· 1·	○	3· 4·
6	3· 4· ·2	○	1·
7	3· 4· ·1	○	2·
8	4· ·3 2·	○	1·
9	4· ·2 ·1	○	·3●
10	○1· ·4	○	·2 ·3
11	·4	○	·1 2· 3·
12	·4 2· 1·	○	3·
13	·4 3· ·2	○	·1
14	3· ·1	○ ·4	·2
15	·3 2·	○	1· ·4
16	·2 ·1	○	·4 ·3●
17	○1·	○	·2 3· ·4
18		○	·1 2· ·3 ·4
19	2· ·1	○	3· 4·
20	3··2	○	·1 4·
21	3· 1·	○	·2 4·
22	○2· ·3	○	4. 1·
23	·24· ·1 ·3	○	
24	4·	○	1··2 ·3
25	4·	○	2· ·3 1●
26	4· 2· 1·	○	3·
27	·4 ·2 3·	○	·1
28	·4 3· 1·	○	2·
29	4· ·3	○	2· 1·
30	2· ·1 ·3 ·4	○	
31		○	·2 1· ·4 ·3

Phenomena of Jupiter's Satellites.

Central Time.

		h	m			
Dec.	1	2	53	P. M.	II	Sh. In.
		3	57	"	II	Tr. In.
		5	29	"	II	Sh. Eg.
		6	33	"	II	*Tr. Eg.
	2	12	39	A. M.	I	*Sh. In.
		1	11	"	I	*Tr. In.
		2	56	"	I	*Sh. Eg.
		3	27	"	I	*Tr. Eg.
		3	05	P. M.	III	Ec. Dis.
		7	51	"	III	*Oc. Re.
		9	50	"	I	*Ec. Dis.
	3	12	34	A. M.	I	*Oc. Re.
		10	03	"	II	Ec. Dis.
		1	36	P. M.	II	Oc. Re.
		7	08	"	I	*Sh. In.
		7	37	"	I	*Tr. In.
		9	24	"	I	*Sh. Eg.
		9	53	"	I	*Tr. Eg.
	4	4	18	"	I	Ec. Dis.
		7	00	"	I	*Oc. Re.
	5	4	11	A. M.	II	*Sh. In.
		5	04	"	II	*Tr. In.
		6	47	"	II	Sh. Eg.
		7	41	"	II	Tr. Eg.
		1	36	P. M.	I	Sh. In.
		2	03	"	I	Tr. In.
		3	53	"	I	Sh. Eg.
		4	19	"	I	Tr. Eg.
	6	5	12	A. M.	III	*Sh. In.
		6	54	"	III	Tr. In.
		7	59	"	III	Sh. Eg.
		9	42	"	III	Tr. Eg.
		10	47	"	I	Ec. Dis.
		1	26	P. M.	I	Oc. Re.
		11	20	"	II	*Ec. Dis.
	7	2	43	A. M.	II	*Oc. Re.
		8	05	"	I	Sh. In.
		8	29	"	I	Tr. In.
		10	21	"	I	Sh. Eg.
		10	45	"	I	Tr. Eg.
	8	5	16	"	I	*Ec. Dis.
		7	52	"	I	Oc. Re.
		5	29	P. M.	II	*Sh. In.
		6	12	"	II	*Tr. In.
		8	05	"	II	*Sh. Eg.
		8	48	"	II	*Tr. Eg.
	9	2	34	A. M.	I	*Sh. In.
		2	55	"	I	*Tr. In.
		4	50	"	I	*Sh. Eg.
		5	11	"	I	*Tr. Eg.
		7	05	P. M.	III	*Ec. Dis.
		11	09	"	III	*Oc. Re.
		11	44	"	I	*Ec. Dis.
	10	2	18	A. M.	I	*Oc. Re.
		12	38	P. M.	II	Ec. Dis.
		3	50	"	II	Oc. Re.
		9	02	"	I	*Sh. In.
		9	21	"	I	*Tr. In.
		11	19	"	I	*Sh. Eg.
		11	37	"	I	*Tr. Eg.
	11	6	13	"	I	*Ec. Dis.
		8	44	"	I	Oc. Re.
Dec.	12	6	47	A. M.	II	Sh. In.
		7	19	"	II	Tr. In.
		9	24	"	II	Sh. Eg.
		9	56	"	II	Tr. Eg.
		3	31	P. M.	I	Sh. In.
		3	47	"	I	Tr. In.
		5	47	"	I	*Sh. Eg.
		6	03	"	I	*Tr. Eg.
	13	9	11	A. M.	III	Sh. In.
		10	10	"	III	Tr. In.
		12	00	M.	III	Sh. Eg.
		12	41	P. M.	I	Ec. Dis.
		12	59	"	III	Tr. Eg.
		3	11	"	I	Oc. Re.
	14	1	56	A. M.	II	*Ec. Dis.
		4	56	"	II	*Oc. Re.
		9	59	"	I	Sh. In.
		10	12	"	I	Tr. In.
		12	16	P. M.	I	Sh. Eg.
		12	28	"	I	Tr. Eg.
	15	7	10	A. M.	I	Ec. Dis.
		9	36	"	I	Oc. Re.
		8	05	P. M.	II	*Sh. In.
		8	27	"	II	*Tr. In.
		10	42	"	II	*Sh. Eg.
		11	03	"	II	*Tr. Eg.
	16	4	28	A. M.	I	*Sh. In.
		4	38	"	I	*Tr. In.
		6	44	"	I	Sh. Eg.
		6	54	"	I	Tr. Eg.
		11	04	P. M.	III	*Ec. Dis.
	17	1	39	A. M.	I	*Ec. Dis.
		2	25	"	III	*Oc. Re.
		4	02	"	I	*Oc. Re.
		3	13	P. M.	II	Ec. Dis.
		6	03	"	II	*Oc. Re.
		10	56	"	I	*Sh. In.
		11	04	"	I	*Tr. In.
	18	1	13	A. M.	I	*Sh. Eg.
		1	20	"	I	*Tr. Eg.
		8	07	P. M.	I	*Ec. Dis.
		10	27	"	I	*Oc. Re.
	19	9	23	A. M.	II	Sh. In.
		9	34	"	II	Tr. In.
		12	00	M.	II	Sh. Eg.
		12	10	P. M.	II	Tr. Eg.
		5	25	"	I	*Sh. In.
		5	30	"	I	*Tr. In.
		7	41	"	I	*Sh. Eg.
		7	46	"	I	*Tr. Eg.
	20	1	11	"	III	Sh. In.
		1	26	"	III	Tr. In.
		2	36	"	I	Ec. Dis.
		4	01	"	III	Sh. Eg.
		4	14	"	III	Tr. Eg.
		4	53	"	I	*Oc. Re.
	21	4	31	A. M.	II	*Ec. Dis.
		7	10	"	II	Oc. Re.
		11	53	"	I	Sh. In.
		11	56	"	I	Tr. In.
		2	10	P. M.	I	Sh. Eg.
		2	12	"	I	Tr. Eg.

	h m			
Dec. 22	9 03	A. M.	I	Oc. Dis.
	11 19	"	I	Oc. Re.
	10 41	P. M.	II	*Tr. In.
	10 41	"	II	*Sh. In.
23	1 18	A. M.	II	*Tr. Eg.
	1 19	"	II	*Sh. Eg.
	6 22	"	I	Tr. In.
	6 22	"	I	Sh. In.
	8 38	"	I	Tr. Eg.
	8 38	"	I	Sh. Eg.
24	2 52	"	III	*Oc. Dis.
	3 29	"	I	*Oc. Dis.
	5 45	"	III	Ec. Re.
	5 45	"	I	Ec. Re.
	5 48	P. M.	II	*Ec. Dis.
	8 16	"	II	*Oc. Re.
25	12 47	A. M.	I	*Tr. In.
	12 51	"	I	*Sh. In.
	3 03	"	I	*Tr. Eg.
	3 06	"	I	*Sh. Eg.
	9 55	P. M.	I	*Oc. Dis.
26	12 14	A. M.	I	*Ec. Re.
	11 48	"	II	Tr. In.
	11 59	"	II	Sh. In.
	2 25	P. M.	II	Tr. Eg.
	2 37	"	II	Sh. Eg.
	7 13	"	I	*Tr. In.
	7 19	"	I	*Sh. In.
	9 29	"	I	*Tr. Eg.
	9 37	P. M.	I	*Sh. Eg.
27	4 21	"	I	Oc. Dis.
Dec. 27	4 41	P. M.	III	*Tr. In.
	5 11	"	III	*Sh. In.
	6 42	"	I	*Ec. Re.
	7 29	"	III	*Tr. Eg.
	8 02	"	III	*Sh. Eg.
28	7 06	A. M.	II	Ec. Dis.
	9 23	"	II	Oc. Re.
	1 39	P. M.	I	Tr. In.
	1 48	"	I	Sh. In.
	3 55	"	I	Tr. Eg.
	4 04	"	I	Sh. Eg.
29	10 47	A. M.	I	Oc. Dis.
	1 11	P. M.	I	Ec. Re.
30	12 55	A. M.	II	*Tr. In.
	1 18	"	II	*Sh. In.
	3 32	"	II	*Tr. Eg.
	3 55	"	II	*Sh. Eg.
	8 05	"	I	Tr. In.
	8 17	"	I	Sh. In.
	10 21	"	I	Tr. Eg.
	10 33	"	I	Sh. Eg.
31	5 13	"	I	*Oc.Dis.
	6 02	"	III	Oc. Dis.
	7 40	"	I	Ec. Re.
	9 46	"	III	Ec. Re.
	8 23	P. M.	II	*Ec. Dis.
	10 30	"	II	*Oc. Re.
Jan. 1	2 31	A. M.	I	*Tr. In.
	2 45	A. M.	I	*Sh. In.
	4 47	"	I	*Tr. Eg.
	5 02	"	I	*Sh. Eg.

NOTE.—In., denotes ingress; Eg., egress; Dis., disappearance; Re., reappearance; Ec., eclipse. Oc., denotes occultation; Tr., transit of the satellite; Sh. transit of the shadow; * Visible at Washington.

Phases and Aspects of the Moon.

		Central Time. d	h m
Apogee	Dec.	2	5 00 P. M.
First Quarter		5	6 15 A. M.
Full Moon		12	1 46 P. M.
Perigee		14	9 00 A. M.
Last Quarter		19	5 16 A. M.
New Moon		26	8 20 P. M.
Apogee		30	5 20 A. M.

Maxima and Minima of Variable Stars.

[From ephemerides by Dr. Loewy in the "Companion to the Observatory," and by Dr. Hartwig in the "Vierteljahrsschrift der Astronomische Geselischaft".]

MAXIMA.

Dec. 3 S Delphini.
4 χ Cygni.
7 S Vulpeculæ.
11 T Canis Min.
11 W Cygni.
12 U Cancri.
15 U Monocerotis.
16 V Monocerotis.
17 S Ceti.
20 R Lyræ.
23 R Vulpeculæ.

MAXIMA CONT.

Dec. 25 U Puppis.
25 U Boötis.
27 S Scorpii.
29 S Libræ.
30 R Hydræ.

MINIMA.

Dec. 2 R Aquilæ.
3 U Piscium.
4 V Cancri.
4 X Boötis.
5 R Lyræ.

MINIMA CONT.

Dec. 6 V Coronæ
6 U Cassiopeiæ.
8 T Virginis.
15 S Virginis.
17 R Centauri.
19 W Tauri.
19 R Sagittæ.
21 S Arietis.
25 T Libræ.
27 S Aquilæ.
27 η Geminorum.
28 V Libræ.

Minima of Variable Stars of the Algol Type.

[Given to the nearest hour in Central Standard Time.]

U CEPHEI.

Alternate Minima.

		h
Dec.	2	12 M.
	7	11 A. M.
	12	11 "
	17	11 "
	22	10 "
	27	10 "

ALGOL.

Alternate Minima.

Dec.	6	10 A. M.
	12	4 "
	17	10 P. M.
	23	3 "
	29	9 A. M.

λ TAURI.

Alternate Minima.

Dec.	7	7 A. M.
	15	5 "
	23	2 A. M.
	30	midn.

R. CANIS MAJORIS.

Every third Minimum.

Dec.	4	2 A. M.
	7	12 M.
	10	10 P. M.
	14	8 A. M.
	17	5 P. M.
	21	3 A. M.
	24	1 P. M.
	27	11 P. M.
	31	8 A. M.

S. CANCRI.

		h
Dec.	6	3 P. M.
	16	2 A. M.
	25	2 P. M.

S. ANTLIÆ.

Every third Minimum.

Dec.	1	10 P. M.
	2	10 "
	3	9 "
	4	8 "
	5	8 "
	6	7 "
	7	6 "
	8	6 "
	9	5 "
	10	4 "
	11	4 "
	12	3 "
	13	2 "
	14	2 "
	15	1 "
	16	12 M.
	17	12 "
	18	11 A. M.
	19	10 "
	20	10 "
	21	9 "
	22	8 "
	23	8 "
	24	7 "
	25	6 "

S. ANTLIÆ Cont.

Every third Minimum.

	h
26	6 A. M.
27	5 "
28	4 "
29	4 "
30	3 "
31	2 "

U. Coronæ.

Alternate Minima.

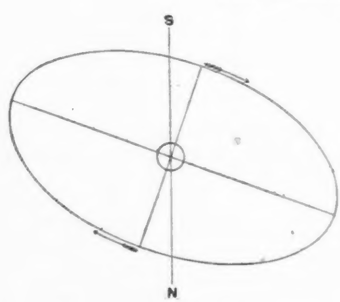
Dec.	1	6 P. M.
	8	4 "
	15	2 "
	22	12 M.
	29	9 A. M.

Y CYGNI.

Alternate Minima.

Dec.	1	6 P. M.
	4	6 "
	7	6 "
	10	6 "
	13	6 "
	16	6 "
	19	6 "
	22	6 "
	25	6 "
	28	6 "
	31	5 "

The Satellite of Neptune.



Apparent orbit of the Satellite of Neptune, as seen in an Inverting Telescope.

Central Times of Greatest Elongations.

Period 5*d* 21°.045.

Northeast.			Northwest.		
Dec.	6	5.5 A. M.	Dec.	3	7.0 A. M.
	12	2.6 "		9	4.1 "
	17	11.7 P. M.		14	1.2 "
	23	8.8 "		20	10.3 P. M.
	29	5.9 "		26	7.4 "

In the diagram the central circle represents the planet and is drawn to the same scale as the orbit of the satellite.

NEWS AND NOTES.

Those who may be in arrears for subscription to volume XIII will greatly oblige the publisher by early settlement of such dues that the books may be promptly closed for the year 1894.

Subscribers are kindly requested to bear in mind the fact that our next number will close volume XIII. It will contain a full general index prepared with the greatest care.

The Transit of Mercury, Nov. 10.—Professor Howe, Director of the Chamberlin Observatory, Denver, Colo., calls our attention to an error in the calculated times of beginning of the transit as given in our last number. The minus sign in the formula for ingress was overlooked by both the computer and the one who checked the work. The Greenwich times should be as follows:

Observatory.	Longitude.	Latitude.	Transit Begins.	Transit Ends.
	° ′	° ′	h m s	h m s
Harvard	71 08	+42 22	3 55 42	9 12 07
Washington	77 03	+38 54	3 55 55	9 12 06
Goodsell	93 09	+44 28	3 55 52	9 12 15
Chamberlin	104 57	+39 41	3 55 59	9 12 17
Lick	121 39	+37 20	3 56 05	9 12 23

North Greenland Aurora Observations.—Those co-operating, and others, will be interested to learn that the records of observations of the aurora made at the station of Mr. Peary in North Greenland the past winter have been received, and that their comparison with similar records from other parts of the Earth is now in progress. It already appears that the conclusions heretofore announced in regard to certain phases of the subject are being substantially confirmed, and that the forces, of which the aurora is the visible expression and type, play an exceedingly important part in many ways. The relations to certain very definite solar conditions, and to thunder storms, and to certain phases of atmospheric control are becoming especially clear. It is fitting that all who have so kindly contributed to the success of the scheme should have assurance that their labor is not in vain It is proposed to continue these observations and if possible make them even more valuable. All data bearing upon the subject will be thankfully received and employed to the best possible advantage. M. A. VEEDER.

Lyons, N. Y., U. S. A., Oct. 18th, 1894.

Since above was mailed 'yesterday,' very fine records have been received from Mr. George Comer, of East Haddam, Conn., made at lat. 63° 55′ N. and long. 90° 20′ W, the nearest the magnetic pole of any thus far at hand.

Transactions of the Royal Irish Academy.—We are in receipt of Part 3, Vol. 30 of the transactions of the Royal Irish Academy. It is a recent publication of micrometrical observations of nebulæ made at the Armagh Observatory, by J. L. E. Dreyer, Ph. D., and read before the Academy, Dec. 11, 1893. These observations were undertaken to supply some of the materials thought necessary to aid in determining the question whether or not the nebulæ are endowed with sensible proper motion. From a knowledge of the proper motion of the stars generally it seems probable that the nebulæ are so endowed, although there is yet wanting the positive proof that a single nebula is affected by such motion. The

question is an important one in the study of the constitution of the Universe, and on this account the brighter nebulæ have been much observed by leading European astronomers. Dr. Dreyer calls attention also to the work of Schönfeld on the positions of many of the fainter nebulæ, indicating that his right ascensions are rather more affected by systematic errors than those of other observers, and on this account that he had thought it useful to re-measure a number of these objects and at the same time to observe some of the brighter nebulæ by means of which a comparison might be made between his own measures and those of Schultz, Vogel· Engelhardt and others.

The work of this catalogue was done by the aid of the 10-inch refractor of the Armagh Observatory, made by Sir Howard Grubb and set up in 1885. The object glass is of excellent quality and the Observatory is furnished with a dome 16 feet in diameter. After describing at length the instrument used and the mode of reducing his observations, Dr. Dreyer speaks of some results obtained as follows:

"In order to establish the relation of my measures to those of other observers, I have compared my results with those of Schonfeld and Schultz with whom I have most objects in common. Their measures are referred to the epoch of 1865.0, and they had therefore first to be corrected for precession to my epoch 1890.0."*

G. C.	*h.*	Corr. to $\Delta\alpha$	Corr. to $\Delta\delta$	Star.
		s	″	
107	46	− .02	− 0.2	
342	128	− .04	− 1.6	$2^{m}\ 15^{s}p$
351	132	+ .01	+ 0.7	
352	131	− .12	+ 1.2	
355	133	− .07	+ 0.7	
363	137	.00	+ 0.4	
470	183	− .02	− 1.6	
549	226	− .09	+ 1.1	
600	262	.00	+ 3.1	$2^{m}\ 15^{s}f$
604	264	+ .10	+ 4.2	
627	II. 619	− .02	+ 0.7	
826	2618	− .01	− 3.9	
1202	IV. 33	.00	+ 3.0	
1225	365	.00	− 5.6	$2^{m}\ 34^{s}p$
1225	365	− .04	− 1.1	$34^{s}p$
1519	444	+ .11	+ 2.0	
1532	450	− .02	0.0	
1567	3095	.00	+ 0.4	
1672	513	− .02	+ 0.2	
1771	564	+ .01	+ 1.2	
1861	604	− .11	− 2.4	
1973	657	+ .02	+ 0.3	
2028	680	.00	+ 0.1	
2030	678	− .10	− 1.1	
2204	755	− .14	− 1.2	
2220	768	+ .04	+ 0.3	
2347	840	+ .05	+ 0.6	
2501	945	− .01	− 0.2	
2757	1112	+ .04	0.0	
2768	1119	+ .04	0.0	
2776	1126	.00	0.0	

G. C.	*h.*	Corr. to $\Delta\alpha$	Corr. to $\Delta\delta$	Star.
		s	″	
2806	1148	− .02	+ 0.1	
2886	1209	− .07	+ 0.2	
2890	1211	− .07	+ 0.2	
3132	1376	− .05	− 0.5	$1^{m}\ 46^{s}f$
3702	1703	+ .01	− 1.2	$13^{s}f$
3702	1703	+ .02	− 1.3	$1^{m}\ 16^{s}f$
3704	1705	− .04	+ 1.9	$2^{m}\ 26^{s}p$
3846	1779	− .03	+ 1.5	
3900	1813	.00	− 0.1	
3964	1857	.00	− 1.0	
4024	3587	− .03	− 2.4	$1^{m}\ 36^{s}f$
4485	2036	+ .01	+ 0.4	
4608	2090	.00	− 1.6	
4632	2102	+ .08	− 0.9	$34^{s}p$
4632	2102	− .01	− 2.1	$1^{m}\ 19^{s}f$
4670	2120	− .04	− 1.6	
4678	2125	− .02	+ 0.8	
4718	2135	− .03	− 0.2	$10^{s}f$
4815	2172	+ .15	+ 1.6	
4879	2199	− .07	− 1.0	
4880	II. 249	− .01	− 0.2	
4883	2201	− .06	− 0.4	
4886	2202	.00	+ 0.1	
4887	2203	.00	+ 0.2	
4892	2205	− .04	− 0.9	
4909	2216	− .03	− 0.7	
4928	2226	+ .05	− 1.5	
4932	2227	− .05	+ 1.0	
4939	2232	− .02	− 0.2	
5029	2282	+ 0.1	.00	

Only such objects were compared which had been measured from the same comparison star. The "Dumb-bell nebula" and *h* 2205 were omitted, as different points appear to have been observed, and here again my few observations of oblique transits were left out. The results are

	$\Delta a \cos \delta$	$\Delta\delta$	Nebulæ.
Schönfeld I—Dreyer	$-0^s.106$	$-0''.05$	27
Schönfeld II—Dreyer	$+0\ .042$	$+0\ .74$	38
Schultz—Dreyer	$+0\ .071$	$+0\ .08$	26

The first two comparisons give the right ascension, Schönfeld I-II $= -0^s.15$, while Schönfeld found by direct comparison between his two series $-0^s.21$ and through comparison with Schultz $-0^s.19$. The reality of the systematic errors in right ascension observations of nebulæ has thus again been confirmed, by the present series of observations, while the differences in declination must be considered as mere accumulations of accidental errors. It appears that I differ less from Schonfeld in right ascension than any previous observer has done, but his right ascensions of nebulæ remain the smallest of any as yet determined. I think, however, that it would at the present moment be premature to attempt to combine the results of all known micrometrical measures in order to form a catalogue of the exact places of nebulæ, but it seems likely that photography might here lead to interesting results, and supply the standard right ascensions which appear so difficult to get hold of."

The Orbit of the Fifth Satellite of Jupiter.—M. Tisserand reports the results of his researches on the orbit of the satellite of Jupiter discovered the 9th of September 1892 by Barnard of the Lick Observatory, in California. This small star is very difficult to observe, nevertheless, the observations of Barnard made with the aid of the greatest telescope actually in existence, are very precise.

M. Tisserand has endeavored to represent these by a circular orbit, a fixed elliptic orbit, and a variable elliptic orbit. The last method gives the most satisfactory results. The eccentricity of the orbit is very small—about 0.01. The ellipse is therefore almost a circle. The major axis makes a complete revolution in five months. This last motion is due to the equatorial protuberance of Jupiter.—Translated from *La Nature* of Oct. 13, 1894.

The Mass of Mercury.—M. Backlund's recent researches on the mass of the planet Mercury, and the acceleration of the mean movement of Encke's comet, are described by M. Callandreau in *Comptes rendus* of October 1. Encke's comet is interesting not only on account of the diminution of its period of revolution (about two hours from one apparition to the next), but also from the fact that its movement is disturbed by Mercury. A discussion of the seven apparitions of the comet between 1871 and 1891 has led M. Backlund to conclude that Mercury has a much smaller mass than has hitherto been ascribed to it. The value obtained is

$$\text{Mass of Mercury} = \frac{1}{9,647,000}$$

It would, therefore, take about 9,700,000 bodies like Mercury to make up the mass of the Sun.

To account for the acceleration of Encke's comet, it has been supposed that a resisting medium of some kind is uniformly distributed round the Sun. M. Backlund, however, thinks that all hypotheses of a continuous resisting medium of uniform density ought to be discarded, and that the resistance is very probably met only in certain regions. This idea is a very plausible one, for, according to Laplace's hypothesis, in the formation of the planets from the solar nebula, all the substance of the rings would not be used up in the process, and some of it would, without doubt, travel along the planetary orbits as clouds of very light

material. It is suggested that Encke's comet passes through nebulous clouds of this kind, and that the resistance they offer causes the observed acceleration of the mean motion.—*Nature*, Oct. 18, 1894.

The Chicago Academy of Sciences.—*Section of Mathematics, Astronomy and Physics.*—The regular monthly meeting was held at the Commerce Club, Auditorium Building, Professor G. W. Hough, President, in the Chair.

Professor Geo. E. Hale read the first paper of the evening on ***Experimental Investigations of the Effective Temperature of the Sun***, by W. E. Wilson and P. L. Gray.

The speaker gave an interesting historical account of the former efforts to determine the effective temperature of the Sun, detailing especially the methods employed by Herschel, Pouillet, Viole, Langley and Rosetti. The principles involved in the experiments were discussed, and illustrations of the apparatus exhibited by means of the lantern. Professor Hale then gave an account of Wilson's work, and pointed out the superior character of his apparatus and the care with which he had corrected for every possible source of error, such as solar and terrestrial atmospheric absorption. The radio-micrometer was explained, and diagrams of work on the melting points of gold, silver, and palladium exhibited.

Wilson's effective temperature of the Sun, 6200 degrees centigrade, was thought to confirm in a general way the value found by Rosetti, which was about 10000 degrees.

In conclusion, Professor Hale thought a great gain had been made by reducing the supposed temperature of the Sun from several million to a few thousand degrees, which he regarded as a first approximation to the truth. A general discussion followed, in which Professor Burnham, Professor Hough, Professor Crew, and others took part.

Professor Burnham, who had just returned from Boston, announced to the Academy that he had tested the 40-inch glass of the Yerkes Observatory at the shops of Alvan G. Clark, and had found it first class.

When tried on double stars like ε Lyræ and β Delphini it gave good images, and when pointed on Vega and α Cygni the field was dark; so that Professor Burnham was satisfied that when finished the glass would be the crowning glory of its illustrious manufacturer.

Professor Wadsworth made a few remarks on Professor Michelson's work with the refractometer, and its application to astronomical measurement. After some general discussion the session adjourned. *6.*

The Perseid Radiant.—Sir: I have only just seen Mr. Denning's letter in your August number on my return from my vacation trip. The question at issue between Mr. Denning and myself is of the simplest possible character, and I hope his personalities will not induce your readers to lose sight of it. It is this: Do Mr. Denning's observations (or any other published observations) *prove* a shifting of the Perseid radiant? If I alleged that I had made observations which led to a different result, his strictures on my skill as an observer and his charge of being located in my feather-bed when he was observing would have some reliance, At present they have none. I accept every one of his published observations as correct. He affirms that these observations prove a shifting of the radiant. I say they do not. The question is not one of observation but of proof. Let Mr. Denning show how the observations recorded by him prove that the radiant shifts, and I will either accept his proof or point out its defects. But I venture

to remind Mr. Denning that assertion is not proof. He may go on repeating that his observations prove or demonstrate the shifting "to the end of time" (if he should so long live) but I do not think he is likely to convince astronomers generally until he condescends to give the details of his proof. Indeed he writes as if the shifting of the radiant was a thing that could be *seen*. Of course it is nothing of the kind. Mr. Denning has recorded fully and accurately what he saw. I do not dispute his catalogue in the least. The question is, what does that catalogue prove? As to the late Dr. Kleiber's computations they proceed on an assumption as to the shape of the meteor-swarm which appears to have been suggested to the author by Mr. Denning's supposed discovery. There is no proof that the swarm has this shape.

I have occupied much of your space in treating of the relations between the proper motions and the spectra of stars. I did not observe either the spectra or the proper motions. I was located in my feather-bed when others were doing so. Consequently, according to Mr. Denning, I knew nothing of what I was writing about and your space has been absolutely wasted.

To be brief, I entirely dispute Mr. Denning's fundamental assumption that no person who is not a practical observer is competent to criticise the inferences drawn by a practical observer from his observations. A very able observer may be a very poor logician and one who never made an observation but understands the elementary principles of the theory of proof may signalise the defects in his argument.

I do not however "aver" that the Perseid radiant does not move. I only assert the "negative result" referred to by Mr. Denning, viz., that its shifting has not been proved. I ask for further observations in order to decide the question whether it shifts or not, and I hope that observers will make these observations regardless of Mr. Denning's *ipse dixit* that they are unnecessary.

Dublin, Oct. 5. W. H. S. MONCK.

P. S. I have seen very few observations of the Perseids of the present year. The only observer who sent me his results (Mr. Milligan of Belfast) writes "No trace of displacement from night to night." He had been on the lookout from about July 24 but did not notice any Perseids until Aug. 4.

The Progress of Astronomical Photography.—All our readers interested in astronomical photography should read H. C. Russell's paper on that theme which was read as the president's address before the Section of Astronomy, Mathematics and Physics of the British Association at its last meeting. We give elsewhere the first part of it. More will appear later.

E. E. Barnard's Present to the Royal Astronomical Society.—At the June meeting of the Royal Astronomical Society it was announced that Professor Barnard of the Lick Observatory, had sent a series of positives of his astronomical photographs as a present. These arrived in due course; and a very beautiful series they are, well worth a visit to the Burlington House specially to see. There are altogether more than 60 glass positives (10 in. by 8 in., 8½ in. by 6½ in.) chiefly of comets, but including one or two of stars, eclipses, etc. The most remarkable are perhaps those of the Brooks' comet, where the tail is in process of being shattered—as though by some cyclone in space. But all are worth careful study, and it is to be hoped that several, if not all, may be published by the Society in some form or other. The whole of the manual labor of taking the originals and copying them has been undertaken by Professor Barnard personally. At the Lick Observatory manual assistance is no more plentiful than some other luxuries which we have cause to regard almost as necessities.—*Observatory for October, 1894.*

Telescope for the Illinois Wesleyan University.—An 18½-inch equatorial reflector provided with driving clock, circles and 2-inch finder has been presented to the Illinois Wesleyan University of Bloomington, Ill., by Mr. A. C. Behr a former resident of that city. It is of the Newtonian form, focal length 10 feet 4 in. Some changes will be made in its mounting and a visual and photographic spectroscope added.

A position micrometer and a time transit have been ordered and the 5-inch refractor used heretofore will be provided with apparatus for viewing the solar prominences. Professor M. P. Lackland will direct the work of the Observatory.

Publications of the Lick Observatory.—Vol. III. 1894 of the publications of the Lick Observatory is received. The present organization for astronomical work at the Observatory is Dr. E. S. Holden, Director and Astronomer, J. M. Schaeberle, E. E. Barnard, W. W. Campbell, R. H. Tucker, Jr., astronomers, Allen H. Cotten, assistant Astronomer and C. D. Perrine, secretary.

The contents of Vol. III are: Selenographical studies, based on negatives of the Moon taken at the Lick Observatory, by Professor L. Weinek. Report on specimens of crown and flint-glass belonging to the Lick Observatory, and manufactured by Messrs. Feil, by Professor C. S. Hastings. Investigation of the glass scale "A" of the Lick Observatory measuring engine, by O. M. Tittmann. Spectroscopic observations of nebulæ made at Mount Hamilton, California, with the 36-inch telescope of the Lick Observatory, by Professor J. E. Keeler.

The introduction by Professor Holden is a description of the optical parts of the 36-inch equatorial with some of the accessories in use in photographic work, a detailed account of work done in lunar photography and a description of some of the very beautiful plates which the Volume contains. The plate representing the region about Mare Crisium when on the termininator of the Moon is certainly the finest representation of that interesting field we have ever seen. The part by Professor Weinek ought to be read by everyone interested in lunar studies. But the especially important part of the volume is by Professor Keeler. We have only space for the summary of his results pretaining to spectroscopic observations of the nebulæ made while he was at the Lick Observatory. We give them in full except the concluding note as follows:

The principal conclusions which have been reached as a result of these investigations are summarized below. Some of them are but a confirmation, by powerful apparatus and exact measurements, of results which had been rendered extremely probable by the labors of other observers. It is on account of the increased weight which is given to these results that they are included in the summary.

1. The normal position of the chief nebular line, or position which the line would have in the spectrum of a nebula at rest relatively to the observer, is $\lambda\, 5007.05 \pm 0.03$ on Rowland's scale.

2. The normal position of the second nebular line on Rowland's scale is $\lambda\, 4959.02 \pm 0.04$. This line is 1.39 ± 0.04 tenth-meters less refrangible than the center of the double line of iron at $\lambda\, 4957.63$.

3. The first and second nebular lines are not represented by absorption lines in the solar spectrum, at least not by any lines sufficiently strong to appear in Rowland's photographic map.

4. The relative brightness of these two lines is, within the limits of error of estimation, the same for all nebulæ. Hence, there is some reason for thinking

that they are due to the same unknown substance, perhaps an element of somewhat the same nature as the so-called helium.

5. The spectrum of the bright-line nebulæ indicates either a high temperature of the gases emitting the light, or a state of strong electrical excitement, and shows that the temperature and pressure are greatly increased at the nucleus.

6. The distance between the Great Nebula of Orion and the Sun is increasing at the rate of 11.0 ± 0.8 miles per second. This result shows that the nebula has little or no motion relatively to the center of figure of the stars from whose proper motions the drift of the solar system has been determined.

7. No relative motion of different parts of the Orion nebula could be detected; the limit of accuracy of the method, for the brighter parts of the nebula, being something like 4 or 5 miles per second.

8. The nebulæ are moving in space with velocities of the same order as those of the stars. A table, showing the measured velocities of particular nebulæ in the line of sight is given elsewhere. Of the nebulæ observed, that having the greatest motion of approach, 40.2 miles per second, is *G. C. 4373*; that having the greatest motion of recession, 30.1 miles per second, is *N. G. C. 6790*. Most of the nebulæ have considerably smaller velocities than these.

9. The only direct attempt to detect rotation of a planetary nebula gave the negative result that there was no differential motion of opposite limbs amounting to as much as 7 or 8 miles a second.

10. The visible spectrum of the nucleus of a planetary nebula has in many cases a strong resemblance to that of stars of the Wolf-Rayet class, although the two principal nebular lines are absent from the latter; and still other differences exist, the importance of which cannot at present be exactly estimated.

The observations which have been described in this paper were intended to form part of a more extended research, which was brought to an end (so far as my own part in it was concerned) by my removal from Mount Hamilton to Allegheny. I have to express my obligations to Professor Holden for every encouragement in the work, and for every facility in its prosecution which the equipment of the Observatory could furnish. I have also to thank Mr. W. W. Campbell, then of the University of Michigan, and now of the Lick Observatory, and Mr. A. O. Leuschner, of the University of California, for their efficient aid in the observations during the summer of 1890.

Observations of Mars.—The current number of the *Observatory* contains a short article in which Mr. Stanley Williams directs attention to certain important features of Mars, which, it will be remembered, is in opposition on Saturday. With regard to the canals or channels, he remarks that a few points upon which observations are desirable are: "How far is the visibility of the canals in different parts of the planet affected by seasonal changes? Their duplication, when does it occur? How long does it last? How does it occur? And again, how far is it subject to seasonal changes?" Mr. Williams commenced observations in the latter part of August, and he found that the plainer canals were conspicuous, and even those of average distinctness could be seen without much difficulty. At the date of writing (September 18) he had observed about thirty of the canals, although only about two-thirds of the planet's face had been examined. Ganges was seen double on August 29, but not so clearly as in 1892. Gehon was also seen plainly double on the same date. Three other canals—Eunostos, Cyclops, and Cerberus—were found distinctly duplicated, and the gemination of Phison was suspected. The observations were made almost exactly at the time of the

summer solstice of Mars' southern hemisphere. Mr. Williams has observed a few small dark spots similar to the "lakes" detected by Professor W. H. Pickering at Arequipa in 1892.

The Algol Variable discovered by Dr. Hartwig, 19 B. D. + 15°.3311, through error in translating from code, it was announced by me to be B. D. + 13°.3115. Will you kindly note the correction? The period of the star is given as 260 seconds less than 2 days. JOHN RITCHIE, JR.

From Canada.—Professor Holden of the Lick Observatory has forwarded the third volume of the reports of the Lick Observatory to the Astronomical and Physical Society of Toronto, Canada, and the work was highly spoken of at a recent meeting of the Toronto society. There are contained in the book some magnificent heliogravure plates, from negatives of the Moon taken at Mount Hamilton, which may be considered the most perfect reproductions ever made.

Mr. A. Elvins, of the Astronomical and Physical Society of Toronto, Canada, has been working industriously on the planet Mars with the 6-inch refractor of the Toronto Observatory. As a result this gentleman has presented to his fellows a number of drawings of the ruddy orb. Colorings of the diverse parts of the Martial disc are faithfully shown in these delineations, but the rectilinear markings, or so-called canals, do not appear. No doubt a 6-inch objective is not sufficiently powerful to reveal those details.

At a recent meeting of the Astronomical and Physical Society of Toronto, Mr. J. R. Collins exhibited some photographs of the magnetic lines of force, made by sprinkling steel filings upon sensitized paper placed over a magnet, finishing by a short exposure to the light. JOHN A. COPLAND.

The Mass of Jupiter.—In *A. N.* 3249, Professor Newcomb has an article which gives valuable information concerning the mass of Jupiter. Its concluding paragraph is as follows:

The following table shows the values, and the relative weights to which I have judged each one entitled. I do not deem it necessary at the present time to give in all detail the considerations which led to the adoption of these weights. I may remark, however, that von Haerdtl's excellent result from the perturbations of Winnecke's comet which has by far the smallest probable error of any determination yet made, has not been assigned a corresponding weight, because of a distrust on my part whether observations on a comet can be considered as having always been made on the centre of gravity of a well defined mass, moving as if its center were a material point subject to the gravitation of the Sun and planets. This distrust seems to me to be amply justified by our general experience of the failure of comets to move in exact accordance with their ephemerides. The mass of Jupiter from

		Wt.
All observations on the satellites	$\mu =$ 1047.82	1
Action on Faye's comet (Möller)	1047.79	1
Action on Themis (Krueger)	1047.54	5
Action on Saturn (Hill)	1047.38	7
Action on Polyhymnia	1047.34	20
Action on Winnecke's comet (v. Haerdtl)	1047.17	10
Weighted mean	1047.35	
	m. e. ± 0.065.	

I propose to regard this mass of Jupiter as a definitive one to be adopted in my work on the planetary theories.

In the interest of the astronomy of the future, it seems very desirable to apply Gill's heliometer method to the continuous observation of a selected number of the minor planets, especially Polyhymnia. I include this planet, although it is to be feared that it can be reached with the heliometer only at opposition near its perihelion.

Brorsen's Comet 1851 III.—This comet first appeared in the month of August, 1851, noving in the constellations of Bootis and Draco. On forty-one evenings observations were made, besides numerous measures of position with micrometers, and many have been the attempts to deduce an accurate orbit. Among these may be mentioned Rümker (*Astr. Nach.*, No. 771), Vogel (*Astr. Nach.*, No. 774), Brorsen (*Astr. Nach.*, No. 775), and Tuttle (*Astr. Journal*, II.), who found parabolic elements, none of which satisfied the observations sufficiently. At a later date Brorsen obtained elliptical elements (*Astr. Nach.*, No. 782), which he compared with all the then known observations. In the communication before us, on a new determination of the orbit of this comet by Dr. Rudolf Spitaler (lxi. *Denkschriften der Math. Naturwiss. Classe der k. Ak. der Wissenschaften*), the writer makes use of some new observations and more accurate places for the comparison stars. To limit this note we will state in a few words the result he has obtained. The most probable parabolic elements after two or three "verbesserungen" were

$$\tau = 1851 \text{ August } 26.2523 \text{ Paris Mean Time.}$$

$$\left.\begin{array}{l} \pi = 316^\circ\ 57'\ 25.7'' \\ \Omega = 223\ \ 40\ \ 21.2 \\ i = \ 38\ \ 12\ \ 57.5 \end{array}\right\} \text{Eq. } 1851.0.$$

$$\log q = 9.9933272$$

An attempt to improve this led to elliptic elements as follows:

$$\tau = 1851 \text{ August } 26.249997 \text{ Paris Mean Time.}$$

$$\left.\begin{array}{l} \pi = 310^\circ\ 57'\ 19.2'' \\ \Omega = 223\ \ 40\ \ 33.9 \\ i = \ 38\ \ 12\ \ 52.9 \end{array}\right\} \text{Eq. } 1851.0.$$

$$\log q = 9.9933235$$

$$e = 0.9999151$$

Both these elements give ephemerides which agree well with the observations, and can be looked upon as accurate within the limit of error of the observations.—*Nature*, Oct. 18, 1894.

The Moon.—The map of the Moon, compiled by Jules A. Colas and published by Messrs. Poole Brothers, of Chicago, is now nearly ready for general sale. We have before called the attention of the readers of this magazine to this important aid to lunar study. The map is now quite complete and a copy is before us, and we can speak of it more definitely than before.

The size of the map is 2 feet by 29 inches. It is mounted on heavy paper which is cloth-lined on the back. The background for the lunar disk is a deep blue, which well represents the color of the sky in full moonlight. The diameter of the disk is a little more than 20 inches. It was made so large that all the prominent features on the surface might be well defined in character and be plainly numbered for easy reference. There are nearly five hundred and fifty such features presented on this map, besides a great number of others of less importance that are never named on any map. The markings that are named are so clearly and neatly

designated that the map is well adapted for class uses in study or lecture, and admirably well suited for the public or private libraries as a reference map.

After considerable study of the shading on the disk to represent the seas, plateaus, mountains, craters and streaks, we are glad to say that the compiler has more nearly represented the true average color of these features, as revealed by the telescope, regardless of lunar phase, than is found on any similar map of the Moon we know of. Although guided by excellent lunar photographs in getting these slightly varying shades, no one knows better than the compiler how difficult it is to correct the errors of a photographic picture which always makes the seas too dark and the plateaus too bright, so as to get a true relation of color for all. In this we think he has succeeded admirably. In photographing the Moon it is very difficult to get any but the most prominent markings on the bottom of the seas or craters, because a length of exposure that would show such details would destroy all markings on the plateaus by over exposure. But in this map the student will be pleased to notice that a great variety of marked and undulating surface is recorded on every sea bottom. This will make the map realistic in a large sense to those who have the aid of a telescope.

The hardest thing to get in such a picture as this, are the streaks that radiate from the great mountains Tycho, Copernicus, Kepler and some others. They are fairly well given, even better than photographs generally show them at any phase, yet the differences between the systems of Copernicus and Kepler are great even in a small telescope. This difference is of no consequence, however, in our present knowledge of the causes of these great markings on the surface of the Moon.

We especially commend this new map of the Moon to all interested in lunar studies, for we do not know of another so good and complete at so small a charge as that asked by the publishers.

An Exercise Book in Algebra, by Matthew S. McCurdy, M. A., contains 175 pages of exercises intended as supplementary drill work and adapted for use with any text of medium grade; it also states a few definitions and brief rules, and thus becomes suitable for use in general review work on the subject of algebra. At its close is a series of specimen examination papers from leading colleges. The neat binding, excellent paper, and clear type make the volume an attractive one; the exercises in their grading and arrangement are the evident work of an experienced teacher. The book is admirably adapted to meet the need which it has in view. Publishers; Leach, Shewell and Sanborn. Price, 60 cents.

Star Names and Meanings.—Richard H. Allen is preparing a book to be titled Star Names and Meanings. We have before us sample pages taken from that part of the book which treats of the constellations. A brief history of the name of the constellation is given first, then follows special notice of each of the prominent stars belonging to the constellation. The Greek letters and the names of the stars are both given in heavy faced type so as to be easily caught on the page by the eye, and the historical meaning of the names is quite fully presented. Nothing is given, in these pages, on the pronunciation of the names of the more familiar stars. This is to be regretted for that is *one* of the lacking things in almost every book about the stars. On the whole these advance leaves promise well and the work will be looked for with interest.

Solid Geometry by Arthur Latham Baker, Ph. D., is a compact, convenient hand-book of the elements of solid geometry. Its notation is new and carefully planned with a view to increased clearness; its demonstrations are brief each being given under the five distinct heads, Notation, To Prove, Constructions, Analysis, Proof. Test exercises are found at the close of each chapter except in the case of the brief closing chapter on conic sections. The author's one aim is to secure for the student a comprehensive, unified, working knowledge of the principles of solid geometry. Publishers; Ginn & Co.

Erratum.—Line 20 from bottom of page 744 should read 3921.32 and 3921.38 instead of 3821.32 and 3821.38.